SCIENCE LECTURES

AT

SOUTH KENSINGTON.

BY

CAPTAIN ABNEY, R.E., F.R.S.
PROFESSOR STOKES.
PROFESSOR ALEX. B. W. KENNEDY, C.E.
F. J. BRAMWELL, ESQ., C.E., F.R.S.
PROFESSOR F. FORBES.
H. C. SORBY, F.R.S.
J. T. BOTTOMLEY, M.A., F.R.S.E.
SYDNEY H. VINES, B.A., B.Sc.
PROFESSOR CAREY FOSTER.

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SCIENCE LECTURES AT SOUTH KENSINGTON.

PHOTOGRAPHY.

TWO LECTURES.

BY CAPT. ABNEY, R.E., F.R.S.

LECTURE I.

IN Fénélon's fables, under the title of *Voyage Supposé*, 1690, a visit to the Isle of Wonders is described, and in that book we read—

"There was no painter in that country, but if anybody wished to have the portrait of a friend, of a picture, a beautiful landscape, or of any other object, water was placed in great basins of gold or silver, and then the object desired to be painted was placed in front of that water. After a while the water froze and became a glass mirror, on which an ineffaceable image remained."

Such was a fancy which, though then of a most improbable nature, presented itself to the mind of a French king's tutor some hundred years ago. In its broad aspect it became a reality when the first Daguerrean image was obtained; though accomplishment was, in a measure, effected in the early asphaltum prints of Nièpce. It is with the realisation of this dream that we have to deal this morning; with that realisation which has furnished people, to be numbered by thousands, with the means of subsistence, and has created fortunes in some few instances; and which has put a new power into the hands of men of science in their investigations.

We shall only cast a rapid glance over the early history of photography, and endeavour to show as far as possible how it was that the great advances in it have been made.

In 1777 Scheele, of Stralsund, in Sweden, was the first who actually carried out investigations into the action of light on silver chloride. Before his time it was well known that "luna cornua" (as silver chloride was termed) blackened in the presence of light, but he arrived at the fact that a chemical change was brought about by the light. He found that silver chloride blackened by this agency on being treated with ammonia yielded up metallic silver, whilst if the exposure took place beneath water a soluble substance was separated, which, when silver nitrate was applied, gave fresh silver chloride. This was an important investigation, but no fruits resulted from it till many years later.

Wedgwood, in 1802, next called attention to photographic action, in a paper read before the Royal Institution, entitled, "An Account of a Method of Copying Paintings on Glass, and of making Profiles by the Agency of Light upon Nitrate of Silver," with Observations by H. Davy. This was the first published account of producing photographs.

Wedgwood used white leather or white paper as a substratum (using the former in preference to the latter) on which was brushed silver nitrate. In his paper he entered into details of his process, and admitted that the images so obtained could not be fixed or rendered permanent. Davy compared Wedgwood's results when using silver nitrate, with those obtained by silver chloride, and found that the latter compound was more susceptible to darkening by the action of light than the former; but in neither case could he fix the images.

The next eminent man who made essays on what we now call photography (or sun-writing) was Nicéphore de Nièpce. He commenced his experiments in 1814, and in 1827 wished to communicate an account of them to the Royal Society of London; his paper was not received, owing to the details of the process being kept a secret. We now know that his process was founded on the action that light produced on bitumen of Judæa (more commonly known, perhaps, as asphaltum); he found that this body when exposed to light, became insoluble in the usual menstrua. Thus, if a thin coating were given to a metal plate and when dry exposed

in a camera to the action of light controlled by a lens, the part acted strongly upon by light would become insoluble, and thus when a solvent was applied (such as naphtha or petroleum) the shadows would be represented by the metal plate, whilst the lights would be formed of the dark resin. The image in this case would be reversed in the character of its shades, unless the black body could be whitened, and the metal blackened. After many experiments with this object in view, Nièpce applied *iodine to the image obtained with the asphaltum.* These bitumen pictures are still in existence, one or more being in the British Museum at the present time.

In 1824 Daguerre, who was devoted to painting, commenced similar experiments to those of Nièpce, each working in secrecy and unknown to the other. In 1826, however through the want of reticence of an optician, who was a acquaintance of both, the fact that each of them was workin in the same direction was learnt by the other, and in 1829 they entered into a kind of partnership. Here it may be that Daguerre first learnt the treatment of metallic plates with iodine, and watched the action that took place in the light, when silver was employed to receive the layer of asphaltum. At any rate to Daguerre belongs the discovery of the action of light on iodide of silver surfaces, and also the merit of producing a picture in the camera with but a short exposure. When I say short, I mean short compared with that given to the bitumen plates, for with such it took six or eight hours to obtain an image. Working with silver plates, which had been subjected to the vapour of iodine, he succeeded at first in obtaining visible images with prolonged exposure; but whilst endeavouring to obtain them in a moderate time he waded through endless experiments, and only chance befriended him at last. It thus occurred:—Having exposed some iodized plates in the camera and obtained no results, he placed them away in a cupboard containing a medley of chemicals. On opening it some time afterwards to procure an old plate to clean for fresh trial he found, much to his astonishment, one of them with a fully developed image upon it. I will not exhaust your patience by detailing how he traced the agency at work which had caused this development. Suffice it to say that it was found to be mercury (which vaporises at ordinary

temperatures) which had collected on the parts acted upon by light.

In June 1839 Daguerre's discovery was announced, and in August of the same year published to the world, and a pension of 6,000 francs per annum given to him by the French Government, whilst at the same time 4,000 francs was allotted to Nièpce the Younger, who had succeeded to the partnership with Daguerre after the death of his uncle.

An outline of the daguerreotype process is as follows :—

A copper plate is silvered by the electro-plating process or any other convenient method, and after very careful cleaning, the silver surface is exposed in the dark to the action of iodine vapours. The iodine combines with the silver, and the metallic surface becomes covered with silver iodide, first canary coloured, then rose, then blue, and so on, *the colour being dependent on the thickness of the layer of silver iodide produced.* When canary-coloured it is supposed that the surface is in the best condition for receiving the impact of light.

It will now be convenient to point out the chemical change that really takes place in the ordinarily employed silver salts when exposed to the action of light. Under certain circumstances, when subject to its impact, silver iodide (which for our purpose we will call Ag_2I_2) throws off one atom of iodine, and we get subiodide of silver (Ag_2I), a slightly black body, left behind. Scheele proved by his experiments that silver chloride ($Ag_2 Cl_2$), when acted upon by light, gave off chlorine (Cl), and we now know that the blackened product is sub-chloride of silver (Ag_2Cl).

Similarly, silver bromide (Ag_2Br_2) is converted into the sub-bromide (Ag_2Br).

Pure and dry silver chloride will change in the light. Pure and dry silver bromide will also change in the light, but not so readily as the chloride. *Pure and dry silver iodide is unaffected by light, unless any body which will take up iodine be present;* even moisture will induce the change if the impact of light be prolonged.

The sensitiveness of both the chloride and bromide is materially increased by the presence of any body which will absorb chlorine and bromine, and in all cases we may lay down the law that the greater its affinity for chlorine, bromine, or iodine, the greater the sensitiveness of the

silver-haloid. It should also be noted that however short the exposure to light may be, the same changes occur in a greater or less number of the molecules though such changes may be invisible to the eye, owing to the preponderance of the unaltered salts.

Now when Daguerre's plates were exposed in the camera for a short time, no visible image was apparent, but, nevertheless, some minute quantity of the Ag_2I_2 was converted into Ag_2I. It was found by Daguerre that such an invisible image had the power of condensing mercury from mercury vapour on the parts forming it, and that metallic lustre was given to it. Thus the Daguerrean image of this white piece of paper would have been represented by mercury and sub-iodide of silver, while this black piece of paper would have been represented by the silver iodide; and when the unaltered iodide is dissolved away, the latter would be represented by the dark-coloured silver, and the former by the lighter amalgam of silver and mercury.

I have here a glass plate silvered by Liebig's process, and we will place it for a couple of minutes in this common deal box,[1] at the bottom of which is a piece of cardboard which has been exposed to iodine vapour during the night. Iodine volatilizes at ordinary temperatures, so by leaving it in the box the surface will be converted into silver iodide by the combination between the metal and the halogen. I now withdraw it, and examining it by candle light, I find it of a delicate canary colour, with a slight tint of rose. It is now in a sensitive condition to ordinary light. I will not waste your time by exposing the plate in the camera, but will place it behind a glass negative picture (what that means we shall learn presently), and expose it for a few seconds in the beam of the electric light. I believe it is sufficiently exposed, so I will take it and hold it in the vapour coming from the mercury (heated over a Bunsen burner to about 150 F.) which is in this small capsule. The image begins to spring out at once, and after a little longer treatment it is fully developed. [The picture was handed round.]

Now silver iodide, as I said, is sensitive to light, that is, light changes it from the iodide to the sub-iodide so long as

[1] The plate was supported on a couple of deal strips laid on the card.

some body is present which can take up the liberated iodine. What have we here, in this Daguerrean process, to do so? We have the metallic silver, for recollect that the iodide is only on the outer surface of the plate. Now I could demonstrate to you the fact that this silver is necessary had I the time. I might have repeated an old experiment, and, having silvered a similar glass plate, and converted the whole of the delicate metallic layer into silver iodide, have then shown its insensitiveness to light, owing to there being nothing to combine with the iodine, which it is anxious to liberate. I will show you by-and-bye another experiment which will illustrate the necessity of an absorbent.

Goddard, a countryman of ours, discovered that by treating the silver plate with bromine after the iodide had been formed, the exposure in the camera necessary to form a mercury-condensing image was shortened from minutes to seconds. Perhaps this was the greatest of all improvements in the daguerreotype process, as it rendered it thoroughly practicable.

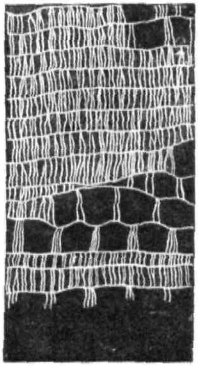
In 1834, whilst Daguerre was working in France on the production of sun pictures, Fox Talbot, a gentleman whom I am glad to say is still living, began experimenting with silver chloride, pursuing the same line of thought as Sir H. Davy, and in June 1839 (six months earlier than the publication of Daguerre's process) he read a paper at the Royal Society on photogenic drawing. This photogenic drawing is really the same photographic printing process that we employ now. Talbot impregnated writing-paper with common salt or sodium chloride, and when dry treated it with washes of silver nitrate, the result being to produce silver chloride[1] in the paper with a little pure silver nitrate ready to take up the chlorine which the darkening chloride would liberate. Ferns, leaves, lace, &c., he copied by this method; more than rivalling the draughtsman in accuracy and rapidity.

Let us suppose that one of the objects to be copied was a piece of black lace. When the lace was laid on the paper those parts beneath the cotton or thread would remain white whilst the ground would be blackened. The paper on the removal of the lace would represent the lace as *white* on a black ground. This picture Talbot termed a negative picture,

[1] $NaCl + AgNO_3 = AgCl + NaNO_3$.

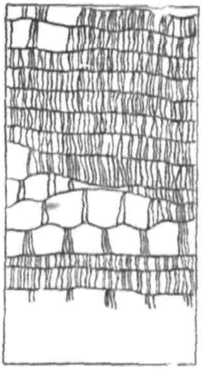
or, as it is now called, a negative. Such a blackened paper with the white image on it could be used to cover a second piece of sensitive paper, which on exposure to light would show the lace as black on a white ground; and this he termed a positive picture. Note the advantage of this process over the Daguerrean, which for each copy of an object required a fresh exposure in the camera.

Hitherto we have dealt exclusively with the method of



Negative.

FIG. 1.



Positive

FIG. 2.

producing silver iodide by the direct contact of the metal with the halogen; but the same results can be obtained by chemical decomposition. Silver iodide may be precipitated by mixing a solution of potassium iodide with silver nitrate.[1] This was the method adopted by Fox Talbot in the calotype process, patented in 1841. He added sufficient potassium iodide to a solution of the silver nitrate to precipitate silver iodide, and then an excess to redissolve it. Such a solution he brushed over a piece of paper, which when dry he washed, leaving on it primrose-coloured silver iodide. In this state the silver compound was insensitive to light, as there was nothing present (besides the paper) to take up iodine. We have here a piece of paper prepared as indicated, and it is now exposed to the strong glare of the

[1] $KI + AgNO_3 = AgI + KNO_3$.

electric light. To render it sensitive before exposure in the camera Talbot brushed over it a solution of silver nitrate and gallic acid, which I will get my assistant to do with our prepared paper. The gallic acid plays a most important part in the process, so much so indeed that I ought to mention that its utility was discovered previously to the Talbot-type process, by the Rev. J. B. Reade, a gentleman who but a few years since has passed away from amongst us. The method of his discovery was systematic. When securing images in the solar microscope he remembered that Wedgwood had produced images on white leather and paper on which had been brushed silver nitrate, and had found that his leather was more sensitive than paper. It occurred to Mr. Reade that the dressing used for the leather might have some important property, so he applied a solution of nutgalls to his paper, and found the necessary exposure to light was greatly shortened. He also discovered that this same nutgall solution had the power of *developing* the image. This, however, was a chance discovery. One day whilst engaged in producing photographically an image of the Trientalis Europæa, he was compelled through circumstances to put on one side the paper which had not been sufficiently exposed to give a visible impression. Placed in the dark, this paper was left till next day, when, on glancing at it, he found a perfectly distinct image. The gallic acid had played a part hitherto not dreamt of.

Reverting to the calotype process, we find that Fox Talbot employed the gallic acid with silver nitrate to render the paper sensitive (the former being what is termed an accelerator); and the discovery that an invisible image could be rendered visible by the same solutions was also utilised, for after exposure to the light the image was brought out by them.

Our own piece of Talbot-type paper is ready, it having been treated as indicated, and the excess of moisture blotted off on blotting paper. After placing it behind the same negative which we employed in illustrating the daguerreotype process, we will expose it to the beam of the electric light. A couple of seconds is a sufficient time to have produced an invisible image, and we will at once proceed to render it visible with a solution of gallic acid and nitrate of silver. This we dab on with a tuft of white wool, and the picture begins to appear. After a little patient manipulation the

whole of the details are brought out, and now we will place it in a dish of water for a while.

This operation of causing the invisible to become visible, how is it effected? We must set ourselves to solve the problem by referring to a kindred action. If a rod of zinc be placed in a strong solution of acetate of lead, by degrees this latter becomes decomposed, and crystals of lead deposit on the rod and completely cover it; but the action does not cease when the covering is effected: the lead solution still keeps depositing the metal, and a beautiful network of leaves formed by metallic crystals is built up. The first particles of lead deposited on the zinc attract other particles from the solution till we have what is known as a lead-tree.

In the development of the image, as our last operation is called, we have simply an example of the laws of crystallisation, like crystals tending to adhere to like, and to be attracted by them. Now in our exposed paper we had some excessively minute portions of silver iodide (Ag_2I_2) reduced to the state of sub-iodide (Ag_2I). Only one silver atom of these more elementary molecules is saturated as it were, and the other is free, and it is this free atom that is capable of attracting metallic silver from a solution of silver nitrate, when this latter is in an unstable state. In the present instance the instability is caused by the gallic acid, for this body tends to absorb oxygen, and as it absorbs oxygen it liberates from the silver nitrate the metallic silver, and as quickly as the separation is effected the free silver atoms attract it. We thus get an image built up on the sub-iodide; for after one small particle of silver has been attracted, it, in its turn, attracts others, as in the case of the lead in the lead-tree. This then is the secret of development; it is the attraction exercised by the sub-iodide for freshly-separated silver.

The developed image is therefore a metallic image; but in order to render it permanent, or perhaps I ought to say, more clear, it was necessary to get rid of all the silver iodide. To Sir J. Herschel belongs the discovery (in 1819) of the solvent property of sodium hyposulphite on the silver chloride, and it was by the application of this salt to the iodide that the desired fixing of the image was effected. It is a matter of surprise that this solvent was not employed at an earlier date.

We will now take from the water our developed print and fix it in a solution of sodium hyposulphite; after washing it will be permanent, or nearly so.

Ten years after the patenting of the calotype by Fox Talbot, a new era arrived in photography. In 1851 was published the collodion process—a process which we use to the present day, and one which there seems to be no chance of superseding, at all events for ordinary work. In the calotype pictures the surface of the paper was found to be too rough to render fine details, and at an early period of experimental photography Sir J. Herschel had suggested the employment of glass as a substitute, and in fact himself had produced pictures on it, for in our Exhibition we find such a picture taken as early as 1839. The method he adopted was to obtain a fine precipitate of silver chloride in water, and at the bottom of the containing vessel to place a glass plate. After a lapse of some time the chloride was deposited with sufficient solidity to render it practicable to remove the glass from the vessel. After flowing over the crust of silver chloride a little silver nitrate he allowed it to dry, and exposed the plate in the camera. The picture I hand round was produced in this manner.

At a later date Nièpce de St. Victor went a step further and employed a film of albumen for holding the sensitive salts of silver *in situ* on glass—an example of an early picture so produced is in the Exhibition; but to Le Gray belongs the honour of suggesting *collodion* as a vehicle to attain the same end. Archer, with whom was associated Dr. Hugh Diamond, however, practically introduced it.

Collodion is a solution of gun-cotton in ether and alcohol, and when properly prepared should leave a transparent film when the solvents evaporate. The iodides, bromides, and chlorides of the alkalies, and also of many of the metals, are soluble in alcohol, and can therefore be introduced into the collodion, and be left in a film of this viscid body when poured over a glass plate. A mere outline of the collodion process is as follows:—A plate is coated with collodion containing an iodide and bromide, as I do this one [shows], and when the ether has evaporated and the film is "set" or become gelatinous, I place it in a dish containing a seven per cent. solution of silver nitrate. On looking at the plate I see the silver compound gradually forming, and after the lapse of about a

minute the film seems nearly opaque. I take our negative picture, and, at each end of it lay a very thin strip of glass and place the sensitive plate upon them. (The strips of glass are used to prevent the surface of the collodion film being abraded by contact with the other plate.) I expose to the electric light for a second. On removing the plate I can see no trace of an image, but I will endeavour to show you its development.

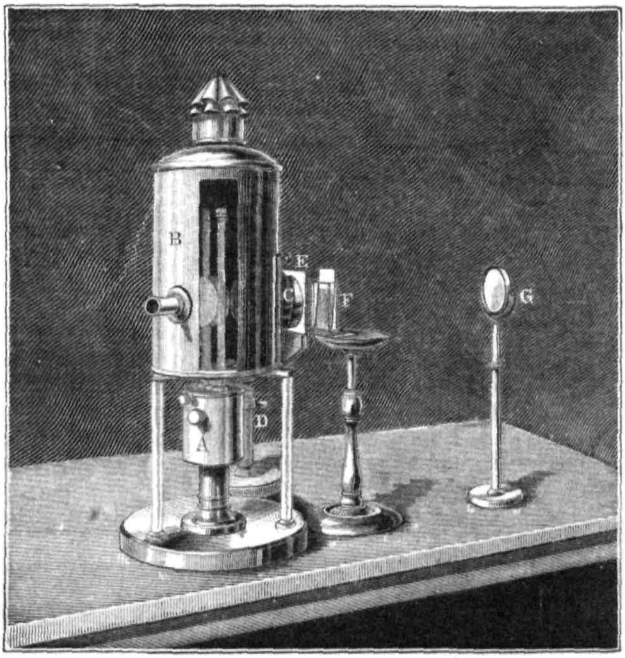


FIG. 8.

Close to the lens (C) of the lantern (B) of the electric light (A) I place a piece of red glass (E) in the clamp (D), for reasons which subsequently will be apparent to you, and immediately in front of that a glass cell (F) containing a solution of ferrous sulphate slightly acidified with acetic acid. I now

use a lens (G) to form an image of the cell on the screen. This being arranged, I dip the glass plate into the solution. You see at first not a sign of any image, but only a semi-opaque film. But now we see a darkening in parts, and a picture is gradually appearing. It gains intensity, and now it is perfect in all its details. We will withdraw it and wash it, and then treat it with a little potassium cyanide, which is also a solvent of many of the compounds of silver. After washing again we will place it in the lantern and throw the finished picture on the screen.

Here then we have a picture produced by the collodion process. You will have noticed that this time the image was brought out by a solution of ferrous sulphate and not of gallic acid. Ferrous sulphate is a greedy absorber of oxygen, and therefore is effective in causing a reduction of silver from the nitrate, with which the plate was impregnated before placing in the cell.

We are now in a position, I think, to make an experiment which I promised at an earlier part of the lecture, viz., to prove to you that silver iodide is unaltered by light unless it has some iodine-absorber present with it. In my hand I hold a glass plate to which is adhering a collodion film containing pure silver iodide and having no excess of silver nitrate. After placing it in the direct rays of the beam from the electric light, I apply a solution of pyrogallic acid and silver nitrate to it, and there is no change apparent. Taking a similarly prepared plate, I apply a small square piece of silver leaf to it, brushing it well on to the film. With this camel's hair brush on another portion I brush a solution of tannin in alcohol, and after warming the glass through its back to cause desiccation, I will expose it for half a minute to the light, behind a negative. We will develop it in the same way as our last picture, using a solution of pyrogallic acid, however, instead of ferrous sulphate. Notice the result—only those portions of the negative appear on our plate which have been coated with the silver or have received the wash of tannin. After fixing our picture I throw it on the screen, and we see our results more perfectly. The transparent parts through which the light passes show where the actinic rays did not affect the silver iodide.

You will notice that the part A is somewhat fainter than B. The former you will recollect was the part on which was

placed the silver leaf, whilst to B was applied the tannin. The reason of the difference is obvious. It is only those particles of the silver iodide in actual contact with the iodine-absorber which can be affected by the light. In the

FIG. 4.

one case the silver leaf was only in contact with the surface particles, in the other the tannin permeated the film. Having thus prepared the ground, we are in a position to understand the preparation of dry plates, as they are called, that is, the preparation of sensitive films in collodion which can be exposed in the dry state.

If a surface of silver iodide were allowed to dry with the free silver nitrate solution from the bath upon it, the latter would crystallize and spoil the film. If we wash away the free silver nitrate it would be insensitive, but if

in its place we applied some iodine absorbent (*after washing*) which is less crystallizable, such as tannin (as we did in our last experiment), salicin, tea, coffee, &c., it will again be sensitive even when dry. The application of such bodies is the foundation of any dry plate process in which the iodide plays a part. At the same time these absorbents are given to the iodide, there are usually added compounds which soak into the film and keep the pores of the collodion open. After washing the exposed film these of course dissolve away, and allow free access to the developer. The same procedure holds good when *both* bromides and iodides are present in the sensitive film, and we may develop with iron or pyrogallic acid and silver, as in the wet collodion process, building up the image on the irritated haloids.

When silver bromide is present alone, or in conjunction with the iodide, however, another method of development may be resorted to, viz., one that is primarily independent of the *deposition* of metallic silver.

Pyrogallic acid has an affinity for bromine as well as for oxygen, and the affinity for both is multiplied manyfold by the addition of an alkali to it. Pyrogallic acid and an alkali such as ammonia (say) form the pyrogallate of the alkali. When such a solution is poured over the exposed bromide film it meets with molecules of the sub-bromide (Ag_2 Br) which appear to be in an unstable state, and ready to part with any of their atoms. The pyrogallate speedily separates the remaining atoms of bromine from them, and leaves the metallic silver behind. When ammonia is the alkali employed with the pyrogallic acid, we have a further action. Over this glass plate, on which is a layer of silver bromide in collodion, I allow a few drops of strong ammonia to trickle. Notice the transparent track they leave. From this we learn that the bromide is soluble in ammonia. Hence, whilst the pyrogallate is still acting in the manner indicated, if there be sufficient ammonia present it dissolves a portion of the silver bromide; this the pyrogallate decomposes, and causes the metal to deposit as in the ordinary development. By this means a greater density is given to the image than would otherwise exist. There is a further density given by a somewhat curious action (apparently catalectic) the cause of which I have not time to explain.

I should state that sometimes the avidity of the pyrogallic acid in the presence of ammonia for bromine is so great that it is found necessary to give it a soluble bromide wherewith to satisfy it. Thus potassium bromide is usually added to the alkaline developer. We will expose a plate prepared with silver bromide behind a negative and develop it by this alkaline method, throwing the resulting picture on the screen. There is no noticeable difference between this image and that developed with the ferrous sulphate in the wet plate.

Now it is not necessary to prepare a sensitive bromide film by immersing the collodionized plate in a solution of silver nitrate; by an artifice we can have the sensitive salts held in suspension in the viscid collodion.

I will very briefly carry you through the operations necessary to produce this emulsion, as it is technically called, of silver bromide. In collodion are dissolved soluble bromides. Silver nitrate dissolved in weak alcohol is added in sufficient quantity to convert it into silver bromide. If rightly carried out, this solid bromide remains in suspension in a very finely divided state in the collodion. The collodion is next poured out into a dish and allowed to become gelatinous by the evaporation of the ether, after which it is well washed (to eliminate all the soluble salts present), and dried. The pellicle is next redissolved in a mixture of ether and alcohol, and we have as a result such a viscous fluid as in this bottle. To prepare a film for exposure in the camera, all that is necessary is to pour it over the surface of a glass or other plate as if it were ordinary collodion. It is equally well acted on when dried as when still moist with the solvents. It is usual to add some bromine absorber to the collodion, but this is not absolutely necessary, though it is generally considered that sensitiveness is increased by so doing.

PHOTOGRAPHY.

LECTURE II.

We left off with the development of the dry plate by the method known as the alkaline process, and this morning we must break into new ground. But yesterday after the lecture a gentleman came to me, and said, "You have not spoken about the latent image." Now what is called the latent image is a thing I hesitate to recognise. I will admit there is an invisible image, but not what should strictly be called a latent image. If we take 10,000 parts of oxide of zinc, say, and mix thoroughly with it one part of lampblack, you will perceive no difference in tint between the pure oxide of zinc and that contaminated with the lampblack; but the lampblack is there nevertheless. So in the same way when we expose for a short time a sensitive plate to the action of light, a change has taken place in certain molecules exactly in the way that would give us a visible image by a longer exposure, the only difference between the one and the other being in the quantity of molecules affected. I think I need scarcely repeat the experiments which showed the development of the invisible image. Suffice it to say that the visible as well as the invisible image, can attract silver from an unstable solution of silver nitrate.

But to-day we have to consider as to the light which is most favourable for photographers, *i.e.* to what rays of light these salts of silver are most sensitive. If you cause a slice of white light to pass through a prism, it is separated into its component rays, and we have what we all know as the spectrum. The spectrum of the light from the incandescent carbons is now thrown on the screen, and if I were to place a large piece of paper impregnated with a sensitive silver salt such as the iodide in that spectrum—and I have taken the iodide with a set purpose—we should notice, on the application of a developing agent, that no change in its molecules had been effected by the red and yellow rays, but that green, blue, violet, and the invisible rays beyond the violet

had been active. The length of the ultra-violet part of the spectrum is equal in length to the whole of the visible spectrum. Mr. Lockyer and Professor Stokes, I think, have told you about these ultra-violet rays, and I am not going to repeat what they have said. Instead of using the electric light we may pass a very thin slice of sunlight through a prism or a couple of prisms, and when we do so, we find a spectrum traversed by black lines, which you have already heard are due to the absorption of metallic and other vapours. Those lines, as you are aware, occupy certain fixed positions in the spectrum, and supposing that we get photographic impressions bounded by any particular line, we should know what part of the spectrum was effective.

Before proceeding further I may show you that with the ordinary silver salts employed the red light is inoperative to produce a picture, whilst the blue light is perfectly capable of so doing. I have here a dry plate prepared with bromo-iodide of silver (I told you yesterday that we mixed bromides with iodides in the collodion, and produced silver bromo-iodide in the film by means of a solution of silver nitrate), and I will expose, for a minute, one half of it behind a negative to the red rays, and the other half for ten seconds to the violet rays. You will see that in the first half of the picture we shall get no results, and in the other half we shall get an image. After it is developed I will throw it on the screen. Here it is after the developer has been applied, and you see the red light is incapable of impressing an image. Before I came yesterday I photographed the spectrum on different silver compounds, to show what rays are capable of producing an image, and when you compare this photograph, taken on the iodide salt, with the carefully coloured drawing hanging on the wall, you will find what I told you was correct about the silver iodide. [The photograph shown on the screen.] The transparent line, which you see at the extreme end of the image, agrees with the line E of the solar spectrum, and when we turn the light on the diagram of the spectrum you will see what position this line occupies in the green. The spectrum goes along through the blue, through the violet, and here we get two well-known lines, called the H lines, also to be seen in the diagram, which are very near the extreme end of the violet portion. You can now see that the photographic action goes far beyond them,

PHOTOGRAPHY.

LECTURE II.

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right into those portions which are usually invisible. To obtain this photograph and those which I shall show you directly, the spectrum was thrown on the sensitive compound after passing through ordinary glass prisms and glass lenses. If I had employed quartz lenses and prisms, or Iceland spar lenses and prisms, I should have been able to obtain an impression much further in the ultra-violet, because glass cuts off these rays to a great extent; but as photographers use glass objectives, I thought it best to show you the spectra as produced through this medium.

I now wish you to compare the spectrum when photographed on other silver compounds, with that already shown. You will notice that the iodide shows the greatest impressibility to those rays which correspond to G, whose wave-length answers to about $4{,}300 \frac{1}{10^{10}}$ metres,[1] at which point it seems to tumble down a precipice in the direction of the violet. The lowest ray which effects it is the E, which corresponds to about 5,200. The maximum effect produced on the bromide seems to take place at about the same wave-length and to diminish more gradually and regularly in both directions. On the chloride the intensity seems to be nearly of the same character as that of the iodide, though the fall in effectiveness is not so marked towards the H line. We may say, then, the gradients of the sensitiveness of the bromide to the spectrum are far less steep than those of the iodide and chloride. The question comes, then, is it not possible to have some compound which shall give a gentler gradient than the bromide, and thus enable us to photograph further in both directions, or, if that be not possible, cannot we change the point of maximum effect to a point nearer towards the red end, by employing a different silver compound, and yet preserve the same gradient, as the bromide? In either case we should be able to photograph further down towards the A line.

Each ray of light, as you doubtless are aware, is caused by a different vibratory motion of the all-space-pervading ether. The waves producing the red rays are longer than those producing the orange, the orange than the green, and so on. Now

[1] In future we shall refer all these wave-lengths to the same scale of $\frac{1}{10^{10}}$ metres.

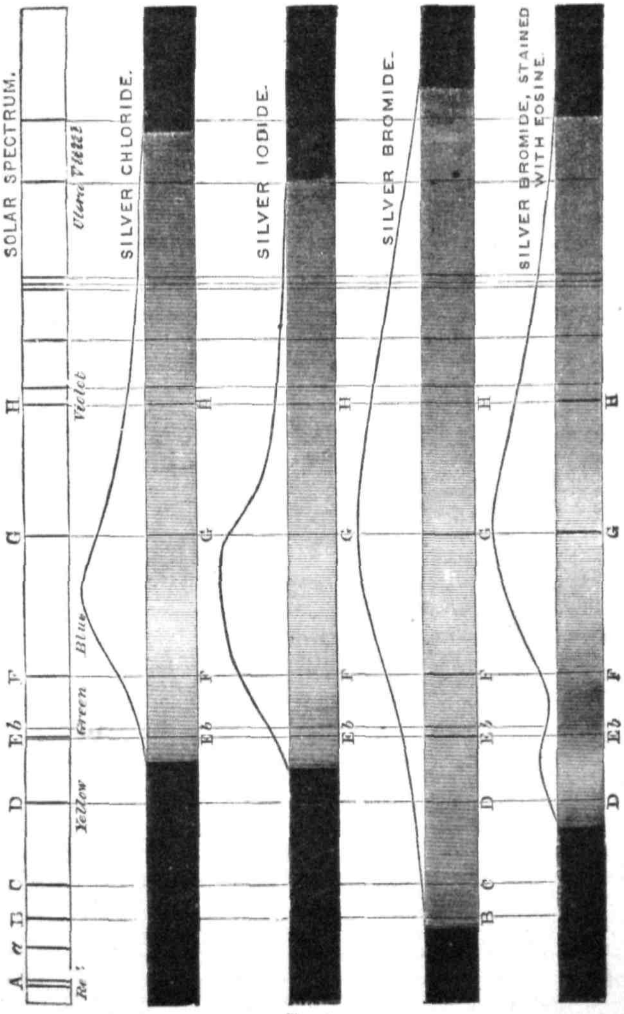
SOLAR SPECTRUM.
SILVER CHLORIDE.
SILVER IODIDE.
SILVER BROMIDE.
SILVER BROMIDE, STAINED WITH EOSINE.
Violet
Blue
Green
Yellow
A
a
B
C
D
Eb
F
G
H

Fig. 5.

when any one of these rays impinges upon a molecule of matter, it meets with an opposition to its motion; it may be that the swing of the wave may be in accord, or nearly so, with the natural swing of the molecule. In such a case we might expect no change to occur in a compound molecule though motion be imparted to it. But it may happen that the beats of the two are of such a nature that a violent internal battering and sifting, as it were, of the molecule will take place. It may be so driven and shaken about, that in order to arrive at some sort of harmonious motion with the wave, it may throw off one of the atoms of which the molecule is composed. It seems probable that to produce photographic sensitiveness in a compound there are two requirements, first, that the molecule should be set in motion by the wave, second, that the motion must be of such a nature that a sifting of its component atoms takes place. The silver iodide[1] molecule is apparently in the greatest internal discord with the blue ray waves, and when they fall upon it, it throws off an iodine atom. If then we can obtain a molecule which can be caused to vibrate by the impact of the red ray waves, and yet be shaken by them, we might expect that such a one would throw off a something to render itself more in tune with that wave.

I give beneath a table of comparative weights of the different molecules of silver chloride, bromide, and iodide.

Ag_2Cl_2	Ag_2Br_2	Ag_2I_2
287	376	470

By loading any of these silver compounds we ought to be able to produce a corresponding change in the limit of internal discord. Thus by loading a silver chloride molecule with a dead weight equal to 89, or the difference between 376 and 287, we ought to cause its limit to be the same as the bromide. Again, we have every reason to believe that by slightly loading the molecule of the bromide we might make its limit of internal discord to be at the A line, or below it; or again, we may expect that by getting a compound of silver which is heavier than 470, the molecular weight of silver iodide, we might obtain similar results. In all cases we must suppose that there is some portion of the molecule that can readily be

[1] The absorption spectrum of silver iodide as well as of bromide shows the greatest absorption to take place in the blue.

shaken off by it when the swing of the wave is sufficiently inharmonious. It should be noted, however, that in these loaded molecules the increased weight may be a dead weight, as it were; that is, the part shaken off may still be chlorine, iodine, or bromine, if the compound contain any or all of these halogens. I now throw on the screen a photograph of the spectrum taken on loaded silver bromide. The bright line in the centre is the extreme limit of visibility, the A line; on the left beyond the A line there are indications of bands of lines, and when you come to examine microscopically a good photograph, you can see that it is particularly rich in lines, a great many of which are due to the absorption of the atmosphere.

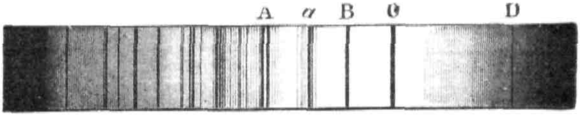

Fig. 6.

To show you that it is not only the solar spectrum that can be photographed by this means, I have here the first photograph taken by myself of the red end of a spectrum of a metal, namely, of calcium. This metal is particularly rich in red rays, together with some rays beyond the extreme limit of visibility. I may remark in passing that by the process here adopted the point of maximum photographic intensity in the spectrum is lowered towards the red. Those two black lines on the left are the last which can be seen, and the lines towards the right lie beyond the A line. Their waves are of such a length that they cannot impress themselves on the nerves of the eye.

I should like to expose a plate coated with this sensitive salt, to show you that there is no illusion about photographing with red rays. I have here such a one which I will expose to the red light only; it will require perhaps a minute's exposure, and I must have it developed as far as possible in the dark, otherwise the image would be veiled. [The developed image was then thrown on the screen.]

Dr. Vogel, of Berlin, has experimented with bromide plates prepared in the silver bath, which, after washing, he flooded with various diluted dyes. He has found and often

correctly, that the film becomes sensitive to the action of those rays which the dyes will absorb. Blue, however, according to his theory, should absorb all the red rays, and thus be chiefly sensitive to the red and yellow. Experiment does not always bear him out in this. I am inclined to think that by the use of these dyes he forms a real compound with silver (for however well you may wash your plates you cannot eliminate all free nitrate of silver), and thus loads his molecules. To explain Vogel's reasoning I will throw the spectrum on the screen, and place a glass cell containing eosine dissolved in alcohol before it; you see there is an absorption in the green, whilst the fluid allows the rays immediately above and below it to pass.

You must bear in mind that where absorption occurs, there *work of some description must be performed*, for the diminution of the amplitude of a wave denotes that energy has been expended.

If we faintly stain a film of silver bromine with eosine, so that the dye absorbs the green rays through its whole thickness, and carries, as it were, the light to every individual molecule in its path, the rays not so rapidly absorbed should not cause such a rapid chemical change. By increasing the strength of the dye on the *washed* plate it is possible to prevent any change in the portion exposed to the green rays, as they are absorbed before they get to the bromide, but if at the same time we increase the silver in the film to such an extent as to allow the whole of the dye to form a compound with it, the effect is still more marked. By this procedure I have been enabled to photograph below the "a" line, with the weighted molecule of bromide. I throw on the screen a spectrum taken on a plate slightly stained with eosine (see Fig. 5), and I will ask you to compare it with the absorption spectrum of the dye and also with the spectra impressed on the other silver compounds. As I said before, I believe that the results obtained with the dyes by Vogel are due to their combination with the residue of silver nitrate left in the film, and what seems to tend to confirm this view is that if the film contain excess of soluble bromide, no results are to be obtained.

Dr. Vogel's researches do not absolutely point to obtaining a method of prolonging the photographic spectrum, yet they tend towards it. You must not accept my explana-

tion of them as correct (though I believe it to be so), as the theory is still in the course of examination by Dr. Vogel, Captain Waterhouse, myself, and others.

We must again travel forward a step, and now we shall find it more easy to understand the production of photographs of a certain kind which we find in the Exhibition. If we take pure silver chloride and expose it to the action of the spectrum sufficiently long to give a visible impression, we find that it extends from the green to far beyond the extreme violet, but that no action takes place in the red. I exhibit here a photograph of the spectrum taken on paper impregnated with silver chloride; the exposure necessary to produce this print was three-quarters of an hour. Another similar piece of paper was taken and slightly darkened in diffused light, and placed in the spectrum. The impression of the blue rays continued, and at the same time there was a browning action taking place where the yellow rays were thrown, whilst at the extremity of the red a decided pink was apparent. Two such spectra I have here, one fixed and the other not fixed; you will perceive that in the former the red tint has been lost, though there is an evidence of decided darkening beyond that due to tint on the paper. In the latter the colours are still extant.

Now conjointly with these results I wish to bring to your notice another experiment that any of you possessing a prism, and a camera, and a looking-glass can repeat, if you have even the smallest photographic knowledge. Take a glass plate and prepare it as if you were going to take a picture, expose it to the daylight and develop it, then "intensify" till you have a perfectly opaque film of silver, and dissolve away the unaltered silver salts, and wash well. Next take some copper chloride and flood the film with it. It will gradually seem to turn to a dirty white by reflected light, and by transmitted light it will be a reddish brown, for the copper will part with half its chlorine to the silver, which there is every reason to think is converted into a mixture of chloride and sub-chloride. Now expose such a plate to the action of the spectrum for ten minutes. You will find that where the violet, blue, and green rays come, you have a darkening of the surface, whilst where the yellow and red rays fall you have a bleaching action.

Again, take another plate, similarly prepared, but give

it a final wash with silver nitrate, and allow it to darken in white light, and then expose it to the spectrum, or beneath red, green, yellow, and blue glasses. In this case we shall find the silver reddens in the red light, becomes greenish in the green, and blue in the blue. I have here a plate that has been so treated and exposed to the different coloured lights, and you can note the colours though they are somewhat spoilt by subsequent experiments. Mark the difference between the two plates: one had a chloride and sub-chloride of silver alone, the other had chloride, sub-chloride, and a chlorine absorbent present.

We may class the result obtained on the silver chloride on paper and that on the last coloured plate as identical, both being exposed under the same conditions. Leaving out, for the present, the theory of the production of the colour, let us examine the fact that a change has been effected by the rays of low refrangibility. We have silver chloride and sub-chloride together in close contact on the surface, and I think we may take it that we have the case of a loaded molecule, whose swing is in discord with the longer waves; that such a discord causes an atom of chlorine to be thrown off (as before explained), and that the atom so separated is from the portion of the compound molecule which by itself would be the sub-chloride. As a result of the impact of the light we should have remaining metallic silver (from the sub-chloride) and unaltered silver chloride. The amount of sub-chloride formed by the preliminary exposure would be small, hence the total amount of reduced silver would be little in comparison with that which would be due to the reduction of the chloride to the sub-chloride by the more refrangible portion of the spectrum. The well-known reversal of the lines of the red end when photographed on iodide or bromide of silver, to which a slight preliminary exposure has been given, can be accounted for in the same way, on the supposition that the silver reduced from the sub-iodide or bromide is not so actively attractive as the sub-iodide or sub-bromide itself. The fact that such reversed photographs are always more or less veiled is rather confirmatory of this view.

With the plate treated with copper chloride alone, and no subsequent addition of silver nitrate and preliminary exposure, the same line of argument still holds good. Part of the sub-chloride is reduced to silver, and chlorine is evolved, the latter

being absorbed by another portion of the sub-chloride, with which it combines to form the white chloride. The minute atoms of reduced silver are shrouded by its whiteness, and we have the consequent appearance of the bleaching of the brownish-coloured film.

The cause of the colours in the paper-print and in the plate requires explanation. The fact that when the unaltered compounds on either of them are dissolved away, the colour vanishes, leaving only that due to silver itself throws a light on the subject.

In a soap bubble the beautiful colours which overspread its surface are caused by the interference of the light reflected from the outer and inner surfaces which are microscopically near each other, and it may be that the colours produced by the spectrum are the results of the interference of the light reflected from the surface of the reduced particles which are held apart by intervening silver chloride.[1] After dissolving out the latter, the particles are brought in contact and the colour disappears.

In the Exhibition we have photographs in colour, representing the solar spectrum, by Becquerel, and one of my objects in leading up so far as I have was to try and give you an explanation of the method of their production. On a bright silver surface silver sub-chloride was formed by voltaic or other means, and a spectrum was caused to fall on a plate so prepared imprinting itself in all its colours. If exposed to the light these spectra fade away and leave nothing behind but a bluish brown plate. Hence it is that they are preserved in closed light-tight cases, and can only be rarely exhibited.

The cause of an action taking place by the impact of the red and yellow rays has already been pointed out; and the vividness of the colours can also be readily accounted for by the same explanation as given to account for those on our paper and collodion film, when it is remembered that there is the reflecting surface of the silver plate itself to aid the interference.

Hitherto we have only spoken about silver compounds being sensitive to light, but nearly all matter is sensitive in one respect or another. Most probably the first action

[1] Silver chloride is really a white transparent substance, as may be proved by fuzing it in a crucible.

with which man was acquainted as photographic action was that of the tanning due to the sun, and in very primitive days no doubt it was more marked than it would be at the present day. The next change most probably would be noticed by the fair sex, who used coloured materials for their dresses, and ladies soon found out that silks, calicoes, or ribbons of certain colours materially changed under the action of light. Here I have three pieces of different coloured materials on which you are able to produce an image by the fading of the dyes. These have been exposed under a negative to the action of light, not heat. The usual explanation about the fading of these colours is that there is something given off like the scent from a rose; but when you see a map absolutely printed by light on them, you can have no doubt as to the action which has produced it. If it were that a sort of essence is given off, a negative placed over these colours whilst in the light would produce no defined result whatever, they would fade equally under it; we therefore cannot help concluding that some chemical change has taken place. Again, you will find most unlikely substances, such as glass, change under the influence of light. My friend Mr. Dallmeyer has some beautiful specimens of glass, which have been altered in this way; flint glass being changed to a yellow colour and crown glass to a purple tint. Again, we know that there are elements which are affected by light, and amongst them I may mention selenium, a body whose resistance to the passage of a current of electricity it has been proved is diminished by the impact of light. Thus in darkness a piece of selenium 1·5 in. × ·5 in. × ·05 in. offered 333000 units of resistance to the passage of the current. Whilst in the diffused light it offered nearly 270000 units. It was also found that the resistance was decreased most in the least refrangible portion of the visible spectrum. In the blue it was only 279000 units, in the yellow 277000 units, and in the red 255000 units of electrical resistance. I have brought this forward to show to you that a simple elementary body may be acted upon by light.

We also find that the colouring matter of flowers and of leaves is affected by light. Mrs. Somerville and Sir John Herschel made a long series of experiments with it. If you take the leaves, say, of common cabbage, and place them in alcohol, a certain coloured resin is extracted, which is known

by the name of chlorophyll. Its solution is red by transmitted, and green by reflected light. Here we have a piece of paper which has been brushed over with this alcoholic solution : the colour is a sort of primrose green. By exposure to light it has become bleached. This bleaching is principally due to the yellow light and not to the blue light, which acts on the salts of silver. Again, if you take the leaves of stocks, common wallflowers, violets, or roses, and treat them with alcohol, you can extract the colouring matter, and if, having brushed it over, you expose it beneath a negative, you will get prints of various colours. If you treat the rose extract with a small quantity of acid and brush it over a sheet of paper and expose it to the light, you will find the natural pink colour intensified and the subsequent change will be increased. Again, take the common violet, treat its extract with ammonia, and it gives you a green solution, but the green colouring matter is bleached by the action of light, and experiment proves that the parts of the spectrum to which the colouring matter is sensitive are not the same as those to which the silver salts are sensitive. There is a wide range of experimental work yet to be undertaken with respect to this colouring matter of flowers.

I should here like to call your attention to the fact that some gaseous bodies as well as solids are affected by light. If, for example, we take hydrogen and chlorine in proper proportions in a glass bulb, and keep them in the dark, no combination takes place ; but if we take such a bulb into sunlight, they combine almost instantaneously, the light causing the atoms to swing in such a way that they mutually attract each other, and form hydrochloric acid. As Dr. Tyndall has shown, it is not heat-waves that cause the atoms to combine, but light-waves ; he enclosed the gases in a collodion balloon, and then caused them to combine by a concentrated light, and the film was found unburnt. In diffused light the combination takes place slowly and without explosion. For similar reasons, if chlorine be passed into water in the daylight, hydrogen is abstracted from the water and hydrochloric acid is formed, the oxygen forming another compound with the chlorine.

The bodies to which I next shall call your attention as sensitive are metallic compounds. Sir John Herschel was the first to investigate the action of light on iron com-

pounds, and to him are due a variety of most interesting processes, examples of one or two of which I shall endeavour to show you.

Whilst mentioning the above great philosopher, I should like to point out to you the instrument with which he operated when experimenting on the effect that light produced on different organic and metallic compounds.

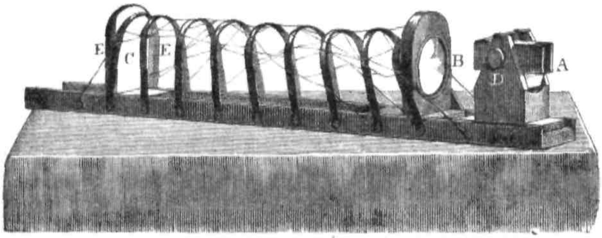

FIG. 7.

A is a glass prism which could be rotated in its frame round an axis D, so that the sun's rays would be dispersed in any given direction. B is a lens defining the spectrum (which, it must be recollected, was a mixed one, and not pure). C the screen on which the spectrum was raised, and on which the compound to be tried was placed. At E was marked a line on which one particular part of the spectrum was invariably caused to fall. When used the frame-work was covered with a black velvet cloth. The absolute results of the experiments on the different compounds are shown in an old book containing a list of Fellows of the Royal Society, over whose distinguished names they have been pasted. This book is perhaps one of the most interesting exhibits in the Loan Collection.

Reverting to the iron salts I may point out that those of them which are in the ferric state are the most readily acted upon by light; the ferrous salts, as a rule, not being sensitive. A variety of ferric salts may be formed, such as ferric chloride, or ferric oxalate; or you may have a compound of ferric citrate with citrate of ammonia, and so on. Of all the compounds of iron, Sir John Herschel found that this latter ferric salt, when employed with the ammonium citrate,

was the most easily operated upon. If we take a piece of paper and brush over it a mixed solution of these two salts, and when dry expose it to light beneath a negative for a short time (about two minutes), we shall obtain a blue image after treating it with a solution of potassium ferri-cyanide. This blue colour can only result from the contact of the potassium ferri-cyanide with some ferrous compound. Here then we have a demonstration of the change effected by light; the ferric compound is reduced to a ferrous state.

I will endeavour to produce such a print before you, but I must tell you that these iron salts are most objectionable for lecture experiments. If you expose a piece of paper which has been coated with this ferric compound, causing an image to be formed of a ferrous salt, and put it away in the dark, it rapidly loses the impression altogether. The ferrous becomes reconverted into the ferric salt. This is exceedingly tantalizing. I had some sheets of prepared paper exposed only this morning, and on developing them by the method already indicated, you see the image is very weak, due to this reactionary cause; had it remained undeveloped a few hours longer we should have had no image at all.

An iron print can also be developed by means of silver nitrate. A ferrous salt of iron will reduce silver nitrate to its metallic state, as already shown in yesterday's lecture; and if I bring a solution of the latter on to the exposed print, you will see that the silver deposits on those parts affected by light.

The next salts to which I must call your attention are the uranium salts, of which there are specimens on the card which you see before you. Uranium nitrate is sensitive to light in the presence of organic matter, being thus reduced to the state of an oxide. This oxide precipitates silver and other metals from their solutions, and with potassium ferri-cyanide forms a nearly insoluble brown compound.

We will now develop a picture with the ferri-cyanide, and you will note its appearance. I have also another photograph printed with uranium, which is now being placed in a solution of silver nitrate, to which a little gallic acid has been added. The silver is gradually being reduced by uranium oxide, and a metallic image is being built up. After passing the paper through sodium hyposulphite and washing, the picture is permanent.

The other most interesting compounds to which I would call your attention are those of vanadium. Professor Roscoe found, during some recent researches, that certain vanadium salts were sensitive to light. Here is the first vanadium print ever produced. It was developed by silver nitrate in a manner similar to that employed with the uranium print.

I am obliged to pass over some other metallic compounds, but I must mention the potassium dichromate, or rather the chromium salts. These salts are the great handmaidens of photographic printing processes at the present day. When you brush a solution of potassium dichromate over paper and expose it to the light, you will find the paper becomes darkened where the light has acted, and an oxide of chromium has been formed, the organic matter in the paper having reduced the potassium dichromate to that state. I have here such a piece of paper which has been exposed under a negative, and washed afterwards; and you see the green coloration of the chromium oxide.

Not only is the potassium dichromate reduced to the state

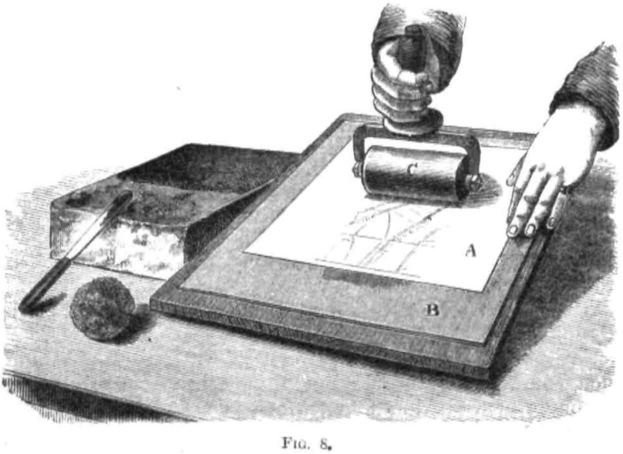

FIG. 8.

of oxide, but it also oxidises the organic matter with which it is in contact. If you take gelatine or any similar colloid body and add to it a solution of potassium dichromate, dry

it, and expose it to the light, you will find the gelatine has undergone a distinct change. First of all, the gelatine has become insoluble in hot or cold water, and in the second place it has become incapable of absorbing water. I have here a sheet of paper (A) coated with gelatine, in which was dissolved potassium dichromate. It has been exposed to light under a negative of a map, and, as I have just told you, it will not absorb water where the lines are printed.

I immerse it in water, place it on this glass plate (B), blot off the excess of moisture, and then roll it with this roller (C), which has been charged with greasy ink. The ink is already beginning to take on the lines; I run the roller briskly over it once or twice to give more ink to them and to remove any adhering superficially, and we have now got a finished map whose lines are formed of the greasy ink; this process of *reproducing plans I have called the papyrotype process.*

In a simple way I want to show you also that gelatine becomes insoluble in hot water by the action of light when it is in contact with potassium dichromate. I have here a piece of gelatinous paper, which has been exposed under the same negative as before, its surface whilst dry has been covered with a thin layer of greasy ink. Now if gelatine becomes insoluble where the light has acted, when I float this paper on hot water, those parts which have been acted upon by light ought to remain on the paper, and those parts which have not been acted on ought to dissolve, carrying the ink with them. I place the uncoated side of the paper on the boiling water, and I notice that an action takes place; where the light has not acted the gelatine is swelling up, showing that it is absorbing water; in other words, I see that the lines forming the image are depressed, and the gelatine around is in relief. I pour a gentle stream of water over the surface, and then I wash away the soluble parts by the application of a sponge. The lines are perfectly distinct, appearing black on a white ground. Both of these properties of chromated gelatine, which I have shown you, are utilised in what we call photolithography. The images formed in the greasy ink can be transferred to a lithographic stone and impressions taken in the ordinary manner. This last process is known as the Southampton method for preparing a photographic transfer for lithography. The first method I showed you is certainly equally as effective. It is also on these two properties of gela-

tine, when in contact with the dichromates—viz., non-absorption of water and insolubility where light has acted—that a variety of other photographic printing processes are

FIG. 9.

founded. On the later reaction is founded the autotype process, where the image is formed absolutely of coloured gelatine, all the parts not acted upon by light having dissolved away; whilst on the former are built up all those processes which produce prints in graduated tints of greasy ink after an image *has been obtained on gelatine that has been hardened and* rendered insoluble in water (though still leaving it capable of absorbing water in the parts not acted upon by light), by the addition of such substances as tannin, chrome alum, &c. As examples of such processes I may mention the heliotype, albertype, and autotype mechanical processes.

I have now come to the end of the time allotted to me, and I trust that the explanations as far as they have gone have been clear; but in treating of such a large subject as photography, it would be necessary for you to listen to me for as many days as you have hours, in order that I might enter into the details of much which I have merely been able to glance at.

THE ABSORPTION OF LIGHT AND THE COLOURS OF NATURAL BODIES.

TWO LECTURES.

BY PROF. STOKES.

LECTURE I.

This subject is one which does not admit very well of experimental illustration before a large class; in fact, with all the appliances of the electric light, I should only be able to show you, comparatively imperfectly, what you can each see for yourselves by experiments which you can make quietly in your own chambers, requiring, I may say, hardly any apparatus at all. The foundation of what I have to say rests on Newton's discovery of the compound nature of white light, with which I presume you are already familiar. You know that when a beam of light is allowed to fall upon a prism, it is decomposed into the different kinds of light of which it consists which are bent round in passing through the prism to a different degree.

Supposing a beam of sunlight reflected horizontally into a room through a small hole and allowed to fall on a prism close by, if the light were of one kind, the beam would be simply bent round as shown in this diagram [referred to], and instead of a circular spot being painted on the wall as at A, it would be as at B. But on making the experiment you have actually an elongated coloured image. The cause of that is, the light is not of one kind, but consists of a variety of kinds differing from one another by the colour with which they impress the eye, and by their refrangibility or capability of being bent

round in passing through a prism, the red rays being bent round the least, and the violet rays the most, while there are kinds of light of all shades of refrangibility between the two extremes. If I were to form a coloured image or spectrum in this simple way it would not be what is called a pure spectrum. Suppose, for simplicity of explanation, we had only two kinds of light, blue and red, differing from one another in refrangibility, then the incident light would be decomposed into those two beams which would each diverge separately from the source of light, or rather from the virtual images of that source, and would be bent round to a very different extent in passing through the prism; consequently if we received them on a screen we should get two circular patches of light, one blue and one red. Now actually, as I have said, you have all intermediate shades of refrangibility, and therefore this compound fan-shaped beam, which passes through the prism, must be regarded as made up of a vast number of cones of light differing from each other in refrangibility, which increases from the red end to the blue end. Consequently if you were to receive the whole on a screen, any one point of the screen would not be illuminated solely by one kind of light, but by all the kinds the refrangibility of which lay within certain limits; in fact there would be a spectrum made up in this way, each circle that we draw representing the section of one of those cones, and each overlapping the neighbouring circles. How then shall we arrange to procure a pure spectrum, and that without loss of light? I say without loss of light, because a very simple mode, in theory, of obtaining a pure spectrum would be to limit a beam of light by one hole, and then by another at a distance; the diverging beam, which passes through the first hole, would be limited by the second, so as to transmit only a very narrow pencil of light, which you might regard as a mere ray, and if you allowed that to fall upon a prism, it would be bent round differently for the different kinds of light of which it consists, so that you would get in that way a pure spectrum, but at an enormous sacrifice of light. How then are we to obtain such a pure spectrum without loss of light? This diagram represents (Fig. A) a beam of sunlight diverging through a small hole, forming a pencil of light. If that were received on a convex lens at a sufficient distance (Fig. B), it would be brought to a focus again on the other side, and would diverge from that

focus afterwards. If we were to take a prism alone, place the prism at a distance from the hole, and in its position of minimum deviation (Fig. C) we should get, if there were two kinds of light only, blue and red, two beams emerging in different directions, the blue (represented in the figure by interrupted lines) being bent round more than the red, and diverging as if they came from two separate points. Now suppose we combine these two pieces of apparatus

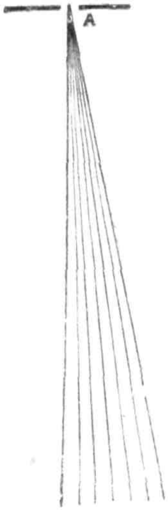

FIG. A.

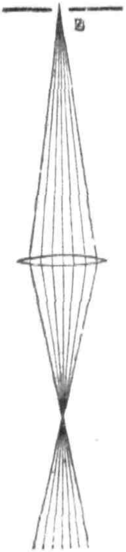

FIG. B.

together, placing the prism at a distance from the hole, and the lens near the prism (Fig. D). Then the prism and the lens each fulfils its own office, the prism causes each conical beam of light to be bent round, but differently, according to the nature of the light, more for the blue than the red; the lens alone collects each of these conical beams, and brings it again to a focus, and so this figure represents what will take place.

If a screen were placed exactly in the focus, and white light containing light of all shades of refrangibility were allowed to fall on the prism, each point of the screen which was illuminated at all would be illuminated by one kind of light only; the different kinds would be separated from one another on the screen, and consequently you would get a pure spectrum, and that without the tremendous loss of light encountered if you employ two holes placed at a distance

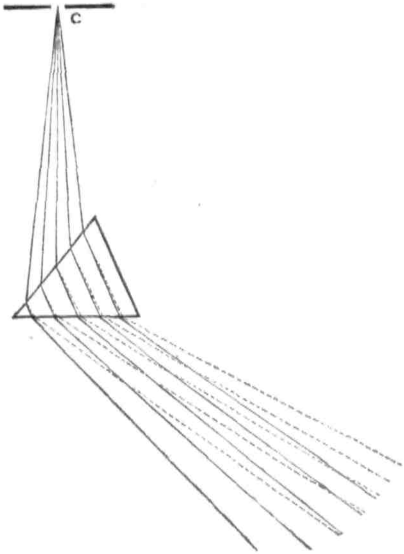

FIG. C.

from each other. The spectrum thus formed, though pure, would be infinitely narrow; but in order to give it breadth you have only to substitute a line of light (in a direction parallel to the edge of the prism) from the point of light; in other words, to transmit the light in the first instance through a narrow slit instead of a small hole. This is the way in which a pure spectrum is generally formed as a matter of principle, though sometimes a mirror is used instead of a lens;

but I will not go further into that, because what I have said is merely an introduction to the use of the prism in the simplest manner possible.

The simplest way to form a pure spectrum experimentally, and a way which suffices, provided you do not want to put objects in the spectrum, but only to see it, is to suppose the

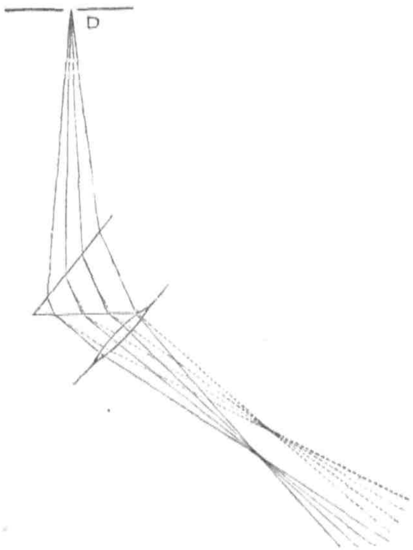

FIG D.

lens to represent your eye, and the screen placed in the focus of the lens to represent the retina.

I said that if the light was passed through a hole it would be brought to a point; but if it came in through a slit, which you may regard as a succession of holes in a direction perpendicular to the plane of the paper, then after passing through the lens it would be brought to a line of light standing out in a direction perpendicular to the plane of the paper, which is supposed to be the plane of refraction for light from the

prismatic image of a candle, and then, keeping that in the field, move off the prism to arm's length, so as to get distinct vision of the prism itself, by moving it about a little you will be able to see whether it is free from veins or not. It should be free from veins, although a prism for merely eye-work need not be of the same excellence as if it were to be used with a telescope.

The more immediate object of my lecture is the coloration of natural objects, and that is best studied in the first instance in the case of clear coloured bodies such as solutions, or coloured glasses. Here is a coloured solution, and if I reflect the skylight through it you will see the colour is blue; but if I add a little more colouring matter to it, it is no longer blue but red. The same effect exactly would be produced if, instead of increasing the quantity of coloured fluid which was mixed with the water, I had increased the thickness of the stratum through which you looked—in fact one is found by experiment to have exactly the same effect as the other. What is the colour of this fluid? If you saw it only in one stage you would say it was blue, and if only in another you would say it was red. It passes in fact from blue to red. What is the cause of that? You must remember that this fluid is illuminated by white light, and white light is not all of the same kind, but is a mixture of portions of light, differing from one another by their refrangibility, and at the same time differing from one another in the coloured impression which they produce upon the eye. In glasses the colouring effect upon the eye simply results from the super-position of various kinds of light which are present. In order to study this phenomenon and the cause of it, we must in the first instance consider what would take place as regards one kind of light alone. If I had one kind of light (and approximately I should get that by a Bunsen flame with a bead of common salt introduced into it), supposing I viewed this through a wedge-shaped vessel which I can slide in front of my eye, if I begin where the thickness is nothing, no effect is produced. In this particular fluid, if I had such a flame before me, I should see at first no effect, and then, as I slid the vessel so as to increase the thickness of fluid looked through, the flame would become weaker and weaker, until I should not be able to see it. The effect is one of a progressive weakening. The longer the path of the light within this coloured

or absorbing medium, the less the quantity of light which escapes; and the law according to which the intensity decreases is very readily obtained by a simple consideration. Suppose first we had a stratum of the fluid of a certain thickness, say one-tenth of an inch, and this produced a certain weakening in the light, or let through a certain percentage. Now if you treated the light which came through to a second stratum, also one-tenth of an inch in thickness, of the same fluid, it would let through the same percentage as before, and so on. When you have to deal with a mass of fluid you may in imagination divide it into strata each of the same thickness. Suppose here is the horizontal surface of a mass of coloured fluid, which we are observing in a vertical direction with white light. The effect of the fluid on light of any particular kind is simply to weaken it. If we divide the fluid into strata of equal thickness, in passing through the first the light is weakened in a certain proportion, depending on the thickness of the stratum. In passing through the second stratum the same percentage of the light will be let through, and so on; so that in passing from stratum to stratum the intensity of the light goes on decreasing in geometric proportion. That is to say, each term of the series expressing the residue bears to the preceding term of the series the same ratio throughout. Consequently when you get far enough into the stratum the light has been so much weakened that it becomes altogether invisible. Theoretically, however great the stratum, there is a quantity which still gets through, but practically, after a certain time, the quantity which gets through is so very small that it may be regarded as nothing at all, and the light is extinguished. Now the rate at which the light is so extinguished depends upon the kind of light which falls upon the coloured stratum. Suppose that rate to be different for different kinds of light, then if there were only two kinds presented to the fluid in the first instance, as it passed on and on, through this absorbing medium, the proportion of these two kinds of light would be continually changing. For the sake of clearness I will suppose there are two kinds of light to start with, blue and red, and that at the beginning blue has an intensity of 100 and the red of 10. There is of course a great predominance of blue over red. Now suppose in passing through a stratum of a certain thickness half the blue light is lost, and

only half transmitted, and that ninety per cent. of the red light is transmitted. Then after passing through the first stratum the intensities will be respectively 50 and 9; after passing the second stratum of the same thickness the intensities will be 25 and 8·1; after the third 12·5 and 7·3; after the fourth 6·2 and 6·6, or about equal; but after passing through the next stratum they will be 3·1 and 5·8; so that although the quantity of red light was so much smaller to begin with, the red is more lasting, and in light which has passed through five of these strata the red now predominates over the blue. Passing through another stratum, the intensity of the blue is reduced to 1·5, whilst the red is 5·2, and so on; so that you see both kinds of light are weakened but the proportion to one another is continually changing. That is a general explanation of what takes place in a fluid such as this [exhibiting an alkaline solution of archil]. I may mention that in almost all coloured fluids there is a *continual change in the colour according to the thickness* of the stratum of liquid, or, which will come to the same thing, according to the strength of the solution. For the sake of simplicity of explanation, I supposed there were only two kinds of light to deal with, which I called red and blue, but in point of fact when the fluid has white light thrown upon it we have an infinite number of kinds of light, and all shades of refrangibility, and each shade of refrangibility must be considered by itself. If we take a certain stratum of a coloured liquid or glass, or whatever it is, then after passing through that stratum the light will be weakened in a proportion which changes continuously in passing from one end of the spectrum to the other. The mode in which the total light passing through the stratum is made up may very conveniently be represented to the eye by a construction given by Sir John Herschel in his treatise on light. Suppose you take a horizontal line and lay distances along that line, or abscissæ, representing the places of the kinds of light which you have under consideration in some standard spectrum, and let lines drawn vertically, or ordinates, represent the intensity of the particular kind of light. If you care merely to know how the quantities go on changing as the light passes deeper and deeper into the absorbing fluid or glass, it will be simplest to take the original intensity as unity throughout, although we know very well that the different parts of the

spectrum are not equally bright. But that is a point which we need not for the moment take into consideration. A horizontal line, then, parallel to the axis of abscissæ, may be supposed to represent for each particular colour, the place of which is defined by the abscissa, the original intensity of that colour. Now after passing through a certain stratum of the medium of a certain thickness, that intensity will be reduced differently for the different colours, and consequently the locus which defines to the eye the composition of the light which is passed through that stratum will be a certain curve, but it will depend on the nature of the medium what the nature of the curve will be. Now I have drawn here [referring to figure] what represents a curve for a green colour in a certain medium. To find how the light will be composed after passing through a second stratum of equal thickness, we have nothing to do but for a sufficient number of abscissæ to take an ordinate which bears to the ordinate of this curve the same ratio that the latter bears to the original ordinate or unity, and so on for additional strata of the same thickness. Thus we get a succession of curves representing to the eye the composition of the light which has passed through successive thicknesses of the medium. You may notice that the quantity of light altogether goes on decreasing; but that is not all, the proportion of the different parts goes on changing as well. In this case, if the opacity of the medium is such as is represented in this curve, the blue or bluish-green light which predominated a little at first will predominate more and more, and the colour of the medium will become a purer and purer green as you look through greater and greater thicknesses of it. Here is another curious curve representing in the same manner the type of light which is transmitted through one of the ordinary blue glasses coloured by oxide of cobalt. I do not pretend that it is an exact representation, but it is an approximate one, and you will see the curious alternations which there are in this case, of comparative opacity and transparency. In this way we can readily understand how it is that the colour of a coloured fluid is continually changing according to the thickness looked through. This phenomenon in its more striking examples is sometimes called dichroism, but as that word has been employed to designate so many phenomena totally different from one

another in their mode of production, I hardly like to employ it.

You have seen in what manner you can readily, and almost without any apparatus, observe a pure spectrum, and how it is modified by the interposition of a coloured body; and I may just mention one or two instances of interesting results which may be obtained in this manner. Sometimes the mode in which a coloured medium attacks the different parts of the spectrum is highly characteristic of the particular fluid that you are employing. Here, for example, is one very characteristic case—the red colouring matter of blood. The spectrum which that gives is represented in the upper part of this figure [referred to]. In order to see the spectrum nothing more is requisite than this: You take a slit of the roughest description—here is one made of wood and tinned iron blackened, and the blood is conveniently held in a test-tube, which you can hold in position by an elastic band. In order to see the spectrum by transmission you have nothing more to do than to hold this against a source of light and look at it. If you use, not a wedge-shaped vessel, but a test-tube, you cannot be sure of not passing over some of the most interesting parts of the phenomena, unless you go step by step, and use several different thicknesses, or, which comes to the same thing, different degrees of dilution. For instance, when a solution of blood is so highly coloured as this, a great part of the spectrum is cut off, and it may be that you will see nothing but a broad black band, whereas, if I had used a weaker solution or a test-tube of smaller diameter, I should have seen certain highly characteristic phenomena of absorption. In order to see these, the solution must be so diluted that it is little more than pink. Then you will see these highly characteristic dark bands of absorption. I know of no substance which can be confounded with blood if you simply take the spectrum of it in this manner, unless possibly an out-of-the-way substance, turacine, the colouring matter found in the red feathers of the wings of the touraco, a bird found at the Cape of Good Hope. If you only looked at the spectrum in one condition, it is possible that the two might be confounded, although hardly so; but if you combine the observation of one of these peculiar spectra with the observation of the effect of re-agents, you get a combination of characters which is such that it is almost impossible to confound any

other substance with the one which you have under your hands. This becomes a mode of discrimination between substances of the utmost value to chemists, but which, strangely, for a long time they altogether neglected, though, since the researches of Kirchhoff and Bunsen, the chemical spectroscope has become an instrument in the hands of almost every chemist.

I may mention one reaction with reference to the colouring matter of blood which is interesting in itself, and will also illustrate what I am saying. You know that the venous and arterial blood differ from each other in colour. If you look at the veins at the wrist you can see the redness of the arterial blood in the arteries which happen to be near enough to the surface, as contrasted with the deeper and darker colour of the venous blood. This difference can be imitated by introducing into a solution of blood a suitable deoxidising agent, which will alter its colour. I have here, in the first instance a solution of protosulphate of iron, and I have added to that tartaric acid, which has the property of preventing the precipitation of many metallic oxides. The colouring matter of blood is immediately decomposed by acid, and therefore you must take care not to introduce acid into the solution. I have rendered this solution alkaline by ammonia without precipitating the iron. This is a strong reducing agent. It is in small quantity almost colourless, and if a little of that is introduced into the colouring matter of blood, which is not decomposed in any reasonable time by ammonia, then immediately the colour is changed into a purple one, and the spectrum is changed in a remarkable manner, as represented in the lower half of the diagram. In lieu of the two dark bands, you have a single band occupying an intermediate position. The fluid is purpler than before, and lets through more blue light. If you have such a solution in a test-tube, and shake it up with air so as to re-oxidise it, you get back the original solution, and you may put it backwards and forwards as often as you like. But I merely mention this as illustrating what you get by using simply a prism without any apparatus at all, and you can see the actual spectrum as shown on the diagram. If a test-tube containing a solution of blood deoxidised in this manner be allowed to stand for some time, it absorbs oxygen from the air, the upper part becomes oxidised, and this oxidation extends deeper and

deeper down, and after a certain time the upper portion of the blood is seen of the scarlet colour, and the under portion of the purple colour. If you then put the test-tube behind a slit, such as I have shown you, and look at it through a prism, you will see the two spectra simultaneously, as represented in the figure.

To take another example, I have here a solution of permanganate of potash. If it is considerably diluted, and you analyse the light transmitted through, you will see a broad dark band in the spectrum. If you have it more diluted, you *obtain a spectrum highly characteristic, in which are seen five* dark bands in the green part of the spectrum. Those are highly characteristic of the permanganates. There is just a trace of a sixth band, which comes in when the solution is stronger. These are alternations of transparency and opacity, not that the fluid is perfectly transparent, but these intervening spaces are really alternations of greater and less absorption. When the quantity present is sufficient, the whole of this region is absorbed, and then the characteristics are lost, because there are a great variety of purple substances which would give a spectrum not very different. In examining a substance you must dilute the solution to make sure of breaking up any such broad dark region, and then you see the dark bands, if any, which are characteristic of the substance. There are other red solutions of manganese which may be obtained, and which agree with the permanganates in being powerfully oxidising agents, and which long ago were confounded by chemists with the permanganates because they have both the purple colour, and are powerfully oxidising agents. For example, if you rub up binoxide of manganese with binoxalate of potash, you obtain one of these purple coloured solutions, though it is not very permanent, which as being a powerful oxidising agent and also of a purple colour, was supposed to contain permanganic acid, but the spectrum instantly shows you it is nothing of the kind. These two examples will suffice to show how valuable the prism is, *even without any other apparatus, as a means of discriminating* between different bodies.

The phenomena of the coloration of natural bodies is best studied, as I said before, in coloured solutions; but I now pass on from that to the colours of natural bodies as commonly presented to us. Let us take, for example, a very common

colour, the green of vegetation, as in grass and leaves in general. What is the cause why a green leaf is green, or why a red poppy is red? It is frequently said that the reason why a red poppy is red and that a white lily is white is, that the lily reflects rays of all kinds, but the poppy reflects only the red ones, and if you place the red poppy in a pure spectrum it is luminous, like a white lily, in the red; but if you place it in the green it will be almost black, whereas the white lily will be brilliantly green. Now the common explanation, properly understood, is true; but it is not the whole truth, and if understood as it is liable to be understood, it is false. It is true that a red poppy reflects red rays, and a white lily reflects rays of all colours; but it is not true that the preference for the red to the green in the one case and the equality of action in the other takes place *in the act of reflection.* It is not a phenomenon of coloration by reflection. The coloured light is reflected, or you would not see it; it is sent out of its course before it enters your eye, and it is true that the light, in its life's history, undergoes reflection; it is not true that it is in the act of reflection that the one colour gets the preference over the other. Here I have some solution of the colouring matter of green leaves in alcohol, and here is some more alcohol, with which I will dilute the former. I have obtained a beautiful green solution, although the green colour is not seen now by reflected, but by transmitted light. As regards the light which falls upon the surface, there is a little white light reflected, just as there would be from water, but very little is reflected from the surface where the fluid is in contact with the glass; the chief portion of that reflected being from the outer surface of the glass itself. You would not see any green at all in it unless there were something placed behind so as to reflect the light backwards. You see there that the colour of the green leaf, as ordinarily seen, is due to the combination of reflection with the phenomena of absorption, or the swallowing up of certain kinds of light when light is sent through a perfectly clear medium. I may illustrate this in another manner. Here is a vessel of water, into which I will pour some blue solution. If I send light through it, it will appear of a deep blue, but if I hinder the light from coming behind, which I can do by putting black cloth behind it, it is simply dark; you do not see the blue colour at all.

Why? Because there is nothing behind to reflect the light. Suppose I make it a little muddy by pouring into it some pounded chalk, you see the blue colour immediately. Why is that? You know that if powdered chalk were put into water it would not colour the fluid. But here each little particle of uncoloured chalk reflects a small quantity of light falling upon it, so that it fulfils the same office as a mirror placed behind the fluid. You may imagine that the particles of chalk are so many minute mirrors capable of reflecting light. If you take any one particle of chalk, say one-tenth of an inch deep, in the liquid, the light from the sky falls upon the fluid, it undergoes absorption in passing through that first tenth of an inch, and then the portion of light which is left is reflected by that little particle of chalk, and passes out again, and so, as regards that single particle, the light which reaches your eye from beneath that depth has itself gone through a stratum of fluid of one-fifth of an inch in thickness, and accordingly you see the colours produced by selective absorption, that is to say, by the absorption of certain kinds of light, which are more greedily devoured by the fluid than the other kinds. This is what takes place in the green leaf, and in the petals of flowers. Let us take the white lily. If the petal of the flower had been merely a sheet of thin glass, you would not have seen that white colour. There would have been a little light reflected from the first surface and the back surface, but the petal is really composed of a vast assemblage of little cells, at each of which partial reflection takes place, so that it resembles some finely-powdered glass, which would form a white powder, because each little surface is capable of reflecting the light, although a single sheet of glass would not be white. The petal of the white lily is just in the condition of the powder. It is full of little cells, full, optically speaking, of irregularities, from each of which a portion of light is reflected, so that, all kinds being reflected alike, and there being nothing in the white lily to cause preferential selection of one over the other—nothing to sift the light, as it were—you get a considerable quantity of light reflected back to the eye, but it is white. What is the difference between that and the red poppy? The red poppy is, as it were, a white lily infused with a red fluid; there is light continually reflected backwards and forwards, just as before, at the surface of the cells; but that light, in going and coming, passes through the

coloured juice of the plant. It is the same thing with a green leaf. The structure is irregular, optically considered ; there are constantly reflections, backwards and forwards, of light, which penetrates a little depth and is reflected, and has to pass through a certain stratum of this colouring matter, to which the name chlorophyll has been given, but which is really a mixture. That is what takes place generally as regards the coloration of bodies ; it is a phenomenon not of reflection, not of selection of one kind of light for more copious reflection than another, but of absorption, or the swallowing up of certain kinds of light. Reflection comes in, in order to enable us to see the light which otherwise would not enter the eye at all, but would go off in another direction.

The spectrum of this green fluid, which is a substance to which I have paid a great deal of attention, is very peculiar. It is a mixture of several substances with closely-allied chemical properties. The peculiar spectrum may be seen in a green leaf itself, if you place it behind a slit and analyse it by transmitted light, or allow a strong light, such as that of the sun, to fall upon it and analyse the reflected light.

Now you will say, Are there no colours in any case produced by reflection? is there no case in which this preferential selection is made? How is it, for instance, if you take a plate of gold ; that reflects light regularly, but the light is coloured yellow? I said the cause of the coloration of bodies in the great bulk of cases was what I have just described, but I did not say that was the sole cause of coloration. The light reflected from gold is in fact coloured ; in the case of gold or of copper there *is* a preferential selection in the act of reflection of one kind of light rather than another, and that preferential selection is not confined to the metals, although it is chiefly in gold and copper that we ordinarily perceive it. There are many cases in which substances which absorb light with intense avidity present a similar reflection of coloured light, and in these substances the connection between the intense opacity of the substance and the coloured reflection can be better studied than in the case of metals, because a metal is, under ordinary circumstances, opaque. Certain of the aniline colours, for instance, show the coloured reflection in a notable manner. These specimens on the table (referring to plates of glass on which solutions of certain aniline colouring matters had been evaporated) were given to me by the late

Sir Charles Wheatstone. He prepared them himself. This is a deep blue or purple by transmitted light, but it is an exceedingly thin film, and by reflected light it has a bronzy appearance. Here is another which is green by reflected light and red by transmitted light. In these cases we see that we have a substance which does exercise a preferential selection for one kind of light as compared with another in the act of reflection. But the light which is so selected for preferential reflection is not at all the light which is chiefly transmitted; on the contrary, it is the very reverse. If we analyse the light transmitted through this red stratum, or through a solution, we shall find that in the green part of the spectrum the substance is more intensely opaque than elsewhere: that is to say, the film must be excessively thin, or the solution excessively dilute, in order that any light at all strong enough to be seen may get through in the green part of the spectrum. The substance is intensely opaque as regards the green, but moderately opaque only as regards the other parts. A solution of this colouring matter does not present this coloured reflection at all. The colouring matter must be excessively concentrated, as it is when a solution of it is dried on glass, in order that this reflection should be shown, and then the kind of light which is more especially reflected in that manner agrees with the kind of light which is intensely absorbed. Those parts of the spectrum which are absorbed with this enormous intensity, so that the dry film is with regard to them as opaque as a film of metal of the same thickness would be, or thereabouts, are reflected as copiously as they would be by a metal, and the colours which are only moderately absorbed are reflected very much as they would be by the glass, and accordingly in the reflected light there is a predominance of those colours which are intensely absorbed. The most remarkable example, that I know of, of the connection between intense absorption and powerful reflection, takes place in the case of crystals of permanganate of potash. These crystals have a bronzy look by reflected light when freshly taken out of the mother liquor, so that the surface is not spoilt by tarnishing, as soon happens from exposure to the atmosphere; the sides of the crystals have a metallic brilliancy, and reflect green light. Now that light agrees with the light reflected from a metal, not only in its copiousness, but also in certain other properties. If I

take light reflected from glass at a certain angle, which is called the angle of polarisation, the reflected light is polarised, and capable of being extinguished by an analyser such as a Nicol's prism. Light reflected from a metal is not polarised at any angle of incidence, though it is partially polarised at an oblique angle. I say partially polarised, but I will leave the explanation of that to my friend Mr. Spottiswoode, who will give a lecture on that subject. Bronzy crystals of permanganate of potash agree in that respect, to a certain extent at least, with the metals; if you examine the light by reflection you find that it is not capable of extinction by analysing under any conditions. If you examine it at such an angle of incidence that a vitreous substance would give you light capable of extinction, the light becomes weaker and of purer green. I have analysed the light reflected by a crystal under these conditions, by a combination of a prism and a Nicol's prism, so as to extinguish what light would have been reflected from glass under similar conditions, and this curious result came out. I must premise that crystals of permanganate of potash are too intensely opaque to allow you to examine them by transmission, but you can make a solution of them and examine that, and it shows these bands of absorption which are shown on the diagram *[referred to]*. Now on examining, in the manner I have mentioned, the green light reflected from the crystals at an angle similar to that at which light reflected from glass would have been quenched by a Nicol's prism, this curious result was obtained; the spectrum was seen to consist of four bright bands, and perhaps a trace of another, the rest of the spectrum being wanting. Now what were the positions of those four bands? When the positions were observed, by referring them to the standard fixed lines of the spectrum, which were seen at the same time, they were in the positions represented in the under part of that figure; they agreed in position with the first four of the five dark bands seen in the transmitted light. The spectrum begins to get comparatively faint in the region of the fifth band of absorption, and there was hardly a chance of seeing the fifth bright band if it had been there; but you see that whereas, as regards transmitted light, the crystals pass alternately through maxima and minima of transparency—alternately from the condition of a vitreous substance to the condition of a metal, as to the avidity with which they absorb the light—corresponding to these

alternations you have also alternations in the character of reflected light; so that you may say the substance is alternately opaque and transparent, comparatively speaking only, as regards the transmitted light, and, corresponding to these alternations, it behaves as regards reflection alternately as a metal and as a vitreous substance. That shows how the coloured reflection, where it does exist—it is a phenomenon, comparatively speaking, rare—is connected with the quasi-metallic opacity of the substance as regards transmission.

You may say that if that be the case the colour of gold ought to be not yellow at all by transmission; nor is it. Gold leaf is thin enough to allow some light to pass through it otherwise than by mere holes, which occur accidentally here and there, and that transmitted light is green. I have here a little chloride of gold in solution. I put a little protosulphate of iron in it, and if the experiment is properly performed you obtain what is not really a solution of gold, but *gold suspended in a state of exceedingly fine division*; and in that way, when the fluid is looked through, you get it distinctly blue, which is the real transmission colour of gold. I have seen the same thing with regard to copper. Dr. Percy gave me a specimen of a very curious glass, which I intended to have brought with me. The ordinary red glasses are coloured by suboxide of copper, which is put over a piece of colourless glass in a film of copper-salt so thin that you do not see any colour at all by light transmitted directly across, but where you look through obliquely you can just see the faintest possible blueness. The film of copper-salt is reduced by a suitable agent to a silicate of suboxide, which gives that beautiful red colour, which is contained in a film thinner than the thinnest paper. In this case the glass was covered with copper in a similar manner, but it was a deep blue by transmitted light, and if you play on any particular spot with a blow-pipe it becomes sensibly colourless. The colouring matter was copper, but in what state? Evidently in this case the reduction necessary for reducing the oxide of copper to suboxide had gone on rather too far, the copper was reduced to the metallic state; you looked *through* the copper, and it was seen to be blue. So that you see that in the same sense in which the coat of an English soldier is red, the colour of gold is blue or green, and the colour of copper is blue. There is the same relation there

as in this aniline glass between reflection and absorption, but whereas in the aniline colours it is commonly the phenomena due to absorption, and the selection of one kind of light over another in the act of transmission, which meet your eye, in the case of the metals gold and copper it is a selection which takes place in the act of reflection which ordinarily presents itself to observation, and the true colour by transmission is only seen under very exceptional circumstances.

FLUORESCENCE.

BY PROFESSOR STOKES.

LECTURE II.

THE subject which I am about to bring before you to day is one which has attracted a great deal of attention for some years back, and in which I have myself had a considerable share. One of the first phenomena discovered in connection with those I have to bring before you was that which Sir David Brewster called the internal dispersion of light, which he first noticed in an alcoholic solution of the green colouring matter of leaves, as mentioned in a paper read before the Royal Society of Edinburgh in 1833,[1] and fully described in a later paper read before the same Society in 1846.[2] I have here a solution of the green colouring matter of leaves which I used in my lecture on absorption, but which I am now going to use for a different purpose. Brewster had occasion to pass a beam of sunlight through this green fluid, and he was surprised to observe the whole of the path of the beam exhibiting a blood-red light. The figure which I have here[3] is intended to represent what could be seen, and I will endeavour to show it presently. This represents a vessel filled with the green fluid and placed on a white ground such as paper, with a glass bottom so that you can see the light through. In looking through you see the green colour of the solution, and there is supposed to be a board standing vertically on its edge containing a lens. The sun's light is reflected horizontally and sent through that lens so as to form a condensed beam, the focus of which lies within the

[1] *Edinburgh Transactions*, xii. 541.

[2] Ibid. xvi. 111 ; or *Phil. Mag.* for June, 1848.

[3] The figures referred to in this lecture are not reproduced, except in two cases, as they are mostly coloured, and would lose much of their significance if merely represented by black and white.

vessel. When that is done, you see the whole path of the beam marked by this blood-red light This is a very curious phenomenon: what is the cause of it? Sir David Brewster seemed to imagine that the ultimate particles of the substance reflected red light somewhat in the manner of finely suspended vermilion. Suppose in fact you had a fluid which was green by transmitted light, and you could manage to form in that an excessively fine mud of vermilion, then it is conceivable that you might get a phenomenon of this kind; I do not say that is the true cause, for it is not. Brewster examined a number of substances, both solutions and solid bodies, in a similar manner, and I may mention one which is described by him in a later paper read before the British Association in 1838, a certain variety of fluorspar. One of the varieties he mentions is a green kind as seen by transmitted light, found at Alston Moor in Cumberland, and I may mention that there is another variety, which usually is purplish by transmitted light, which abounds in Mr. Beaumont's lead mines at Allenhead, which shows the phenomenon even better. This kind of fluorspar shows a deep blue light in certain aspects. You see that to perfection, if you plunge the spar into water, because then you get rid in a great measure of the light reflected from the surface. When a condensed beam of sunlight is admitted into the crystal, the path of it is marked by a blue light. It is not, however, continuous, like the red light in the green fluid, but it occurs in strata parallel to the nearest faces of the cube. Evidently it depends upon something which took place during the growth of the crystal. Possibly it may have crystallized thousands of years ago, we know not how long, out of a solution, the nature of which gradually changed as the crystal grew, and some substance probably was taken up by the crystal, to which this effect is due.

Some years later Sir John Herschel published a paper in the *Philosophical Transactions*, "On a case of superficial colour in a colourless liquid," which was shortly afterwards followed by a paper "On the epipolic dispersion of light.[1]" Quinine as you know is very much used in medicine and when a solution of quinine is formed, tolerably dilute, in water acidulated

[1] *Philosophical Transactions*, Jan. 1845, pp. 143, 147.

with sulphuric acid, by transmitted light the fluid looks very much like water, but it exhibits in certain aspects a blue colour. You will not perhaps very well see it here, but this is such a common fluid, easily obtained by anyone, that it is almost sufficient to mention the appearance. What is remarkable about this blue colour is that (unless the solution be excessively dilute) it is mainly concentrated, and occurs in an exceedingly narrow stratum adjacent to the surface by which the light enters the fluid. This diagram [referred to] represents the appearance.

This [referring to figure] is supposed to be a section of a tumbler containing the solution, placed on a black ground, and tolerably near a window from which light is coming in approximately horizontally. When you look down from above you see that the side of the fluid next the window is marked by this bluish colour, and when you hold the eye almost in a prolongation of the anterior surface of the fluid you see this blue stratum very much foreshortened and thereby increased in intensity, because the fluid itself is transparent like water, and the blue light which appears, whatever may be its cause, is seen perfectly well through it. When you look down in an oblique direction you see it much less intense. It is seen in perfection on allowing the light to shine from above, holding the eye a shade below the level of the upper surface, and putting a black object to make a dark background.

Now what is the nature of this blue light? Sir John Herschel tried various experiments on it. He analysed it by a prism, and obtained a continuous spectrum. He noticed, however, that he did not see the Fraunhofer lines in the spectrum; but whether they were really absent, or that he did not see them because the light was not strong enough, he does not seem quite decided. He noticed also that the blue light exhibited no trace of polarisation. He examined further the light transmitted through the solution to see what blue rays were taken out of it. Naturally he was led to scrutinise more particularly the blue part of the spectrum, but apparently the blue part of the spectrum was like the blue part of the spectrum of light which had come through simple water; there was nothing particular to be seen in it to account for the phenomenon. Possibly owever, if this superficial colour is produced once only,

the quantity of light removed from the spectrum may not be sufficient to show any dark bands of absorption in the blue region, but if you repeated the process on the light, making it pass through different vessels in succession giving out this blue stratum at the surface of each, perhaps then you would have sufficiently weakened the blue of the transmitted spectrum to show what particular rays were taken out by the fluid. Sir John Herschel however observed that when the light had passed through a thin stratum of the fluid in the first instance, though it resembled ordinary light when it came out, it had lost its power, for some reason or other, of producing this phenomenon. What the power was did not at the time further appear. Sir John Herschel called the phenomenon *epipolic dispersion* from a Greek word signifying surface, and he called the light which having passed through a moderate thickness of solution of quinine had been shorn of the power of producing that effect, *epipolized.* In one of his experiments he had occasion to throw sunlight vertically downwards on the fluid, and in that case, the light being pretty strong, he observed the blue colour extending to a depth of half an inch or more into the solution. It was much stronger at the surface, but extended a considerable way down.

After the appearance of Sir John Herschel's paper, Sir David Brewster took up the subject and examined this particular fluid, the solution of quinine, as he had done the solution of the green colouring matter of leaves and fluorspar, and various solutions,[1] and he found that when a beam of sunlight, concentrated by a lens, was admitted into a solution of the quinine in dilute sulphuric acid, the whole of the path of the beam was marked by this blue light. At the same time, if you repeat the experiment, you will see at once that the blue colour is decidedly more copious in the immediate neighbourhood of the first surface. He further examined the beam as to its polarisation, by viewing it through a rhomb of calcareous spar, and stated that a considerable portion of it, consisting chiefly of the less refrangible of its rays, was polarised in the plane of reflection, while the greater part, constituting an intensely blue beam, was found to be unpolarised. It is almost impossible to get

[1] See the paper already referred to, *Edinburgh Transactions*, xvi., or *Phil. Mag.* June 1848.

a fluid or solution like this perfectly free from motes, and the motes which are present will reflect a certain quantity of white light, and that light is principally polarised when looked down on vertically from above in a plane passing through the beam. When the beam is viewed by light polarised in the perpendicular plane, any light which would be reflected from motes is nearly got rid of, and the blue light is seen in its purity.

In this mode of observation it clearly appeared that the solution of quinine belonged to the class of bodies in which Sir David Brewster had discovered what he called internal dispersion. Among them there is a kind of glass which exhibits it in a very remarkable degree Here is a specimen of the glass ; it is coloured by sesqui-oxide of uranium. He noticed in this case, that the whole of the beam was unpolarised, or, as he expressed it, possessed a *quaquaversus* polarisation.

It was twenty five years ago last Easter, when I was preparing for my optical lectures in Cambridge, that, having had my attention directed by a friend to this solution, I procured some, and I was greatly struck with the remarkable appearance of the phenomenon, and the question occurred to me what was the cause of it. Now for my own part I had the fullest confidence in the doctrine that the light belonging to a given part of the spectrum is homogeneous, or all of the same kind. I am not now speaking of polarisation. It has been supposed by some, by Sir David Brewster, for instance, that the light of a given part of the spectrum, although no longer decomposed by the prism, might be decomposable by other means, for example by the use of absorbing media; and he thought he had obtained white light from a particular part of the spectrum by the use of a suitable absorbing medium. This has since been proved to be merely an illusion of contrast. Having, as I said, felt the fullest confidence in the principle that the light of a given part of the spectrum is homogeneous, I felt little doubt that if the principle were faithfully followed out, it would lead to a solution of the problem what the nature of the light which produced this effect was. At first I took for granted that the blue light, which the prism shows to be heterogeneous and not mere prismatic blue, a compound of various colours, a little red, more green, but with a

predominance of blue, could only have come from light of the same refrangibility in the incident beam; but in following out the consequences of that, I was led of necessity into utter extravagances as regards the cause of the phenomena, extravagances which did not appear to have any resemblance to truth ; and on further reflection it occurred to me that perhaps after all this blue colour was not produced by the blue rays of the spectrum at all, but by other rays. We know that the spectrum contains rays which are invisible, but in all other respects behave exactly like light. But the invisibility is a mere accident, so to speak, depending on the organization of the human eye ; and the eyes of animals in general are probably very much like the human eye in this respect, although it is quite possible that certain animals may see rays which we do not; but that we cannot well make out. We know that light contains besides the visible rays others which are invisible ; some less refrangible than the red, and others more refrangible than the violet. We know that the latter show themselves especially by their chemical effect, for example, on a properly prepared photographic plate, and abound in sunlight and daylight, which show strongly the blue light given out by quinine solutions, while lamp-light, which we know to be poor in those rays, shows it but feebly.

Then it occurred to me that perhaps this blue colour which the solution gives out is after all the work of the invisible rays which we know to accompany the visible ones. If we suppose this fluid, which looks colourless like water, to be excessively opaque, inky black as it were, with regard to the invisible rays of high refrangibility, and if we further suppose that these invisible rays are capable of so working on the fluid as to cause it to give out visible light, then the explanation of epipolic dispersion and the nature of epipolised light will be perfectly plain.

Now as we have been going on for some way without any experiment, I will have the room darkened, and will endeavour to show you one or two. I will take the original fluid in which the first phenomenon was discovered Here is this green solution of leaves, and you will be able to see a little of the red light. Here is the yellow glass I spoke of, and in this the green band will be seen very copiously ; here, again, is a solution of quinine, and with this I will

endeavour to show you the fundamental experiment of Sir John Herschel, that this epipolic dispersion is a thing which cannot be repeated ; once done the light is shorn of the power of producing it. I send a beam emanating from the electric lamp horizontally, and reflect it downwards on the surface by a mirror. There is a second vessel of the solution which I place floating in the other, and the blue stratum is seen at the upper surface of the fluid in the upper vessel, while the quinine in the lower vessel shows nothing of it. There is still the general diffused light, but this intense blue stratum is wanting altogether beneath the bottom of the upper vessel. Now I will replace the upper beaker by one of water, when you will see the difference. Just below the bottom of the upper vessel you have this intense blue stratum, whereas the light passing through the water in the upper beaker shows nothing of the kind. That is evidence that when the blue stratum is formed once, the *light is shorn of the power of forming it again.*

As the room is darkened I will show one or two more examples of highly fluorescent solutious before I proceed. Here is a solution obtained in a particular manner, which I will mention by and by, from the bark of the horse-chestnut.

I told you what occurred to me as to the cause of the phenomenon, and it was easy to test it I will not go through all the experiments I tried. but pass at once to what you may regard as the fundamental experiment. Suppose we form a pure spectrum in the ordinary way; that we reflect the sunlight horizontally into a darkened room, passing it through a slit, and at a distance from the slit place one or more prisms combined with a lens, so as to form at the focus of the lens conjugate to the slit a pure spectrum. If you were to receive the pure spectrum on a screen, you would see the various colours, and if it were pure enough you would see the principal Fraunhofer lines. Now suppose instead of a screen you receive it on this colourless fluid. The appearance is rudely represented on this diagram.

I must mention that portions of the diagram are merely diagrammatic, namely, the top and bottom. The middle part represents what you actually see when you look down on the solution, but the top represents what you would see

on a screen if you placed a screen there to receive the rays. This figure represents what is seen horizontally. The rays enter the solution of quinine, and the red, orange, green, and blue rays pass through it as through water, and come out on the other side, and if you received them on a screen, you would perceive the spectrum unmodified; but when you get to about the beginning of the violet, the path of the rays within the fluid is marked by a beautiful sky-blue light, which at first extends right across the vessel, and then the extent falls off as you get towards the end of the violet. But the light does not stop there. This

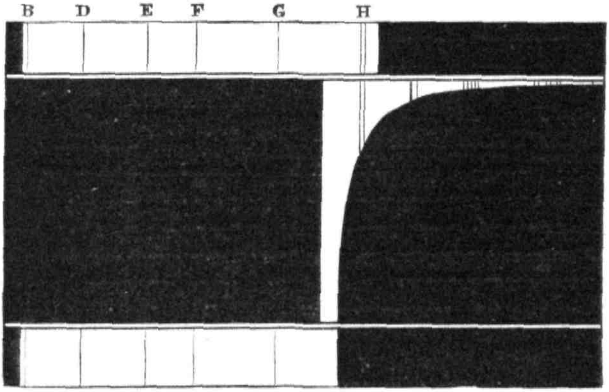

Fig. 1.

blue light extends far beyond the violet into a region of the spectrum which contains only invisible rays; and if the spectrum is pure you see not only light there, but interruptions which are of the same nature as the Fraunhofer lines which are seen in the visible spectrum. On a screen of paper placed vertically to receive the light, the Fraunhofer lines would be seen as distinct vertical lines when the paper was in focus, and would be seen a little imperfectly if the paper were a little out of focus. If you imagine the paper placed in one position, and moved towards or from the light, any particular dark line may be considered as

having a *locus* in space, and that *locus* in space of one of the dark lines is what you may call a dark plane. At a considerable distance from the focus it would diverge out, forming a sort of wedge. These dark planes are seen as interruptions to the blue light. What are the Fraunhofer lines? They are parts of the spectrum when the light is missing, and consequently any effect the light is capable of producing will be missing too. Therefore when we get to the invisible region, if the invisible light is missing the visible light which that might be capable of producing will be missing too, and therefore you will see interruptions in this continuous mass of blue light. It constitutes a very striking experiment with sunlight, when you form an approximately pure spectrum by placing some prisms close to a pretty broad slit, and take a tube filled with the solution of quinine, or a prepared solution from horse chestnut bark, and make it pass through the different parts of the spectrum in succession, beginning at the red end. At first it looks like water by transmitted light; the light rays are transmitted through it as they would be through water, on to the blue, but when we get on to the violet then the whole of the tube is lit up with this faint ghostly sort of blue light; when you have got beyond the visible rays altogether the tube is still lit up with the blue light. This shows that the explanation which occurred to me is really the true one, and that this blue colour is produced, not by the blue rays of the spectrum at all, but by other rays altogether; that rays of one refrangibility act on the fluid in such a manner as to cause it to give out rays of a different refrangibility altogether, or rather, of a different series of refrangibilities, because if you examine a small portion of this blue light, produced by rays of one definite refrangibility only, as you may do by putting the solution in a pure spectrum and placing a slit in front, so as to let only a narrow strip of the incident rays shine on the fluid, and then analysing the narrow beam from above by a prism applied to the eye, you will find that the light is not homogeneous at all. Nor again is there anything in the phenomenon which recalls to the mind the acoustic phenomenon of harmonics; the light is perfectly heterogeneous.

I have on one of these diagrams another mode exhibited

of examining the refrangibility of the light which is emitted in this manner. This part is supposed to represent what would be seen if you place a screen to receive the incident rays, but which is not seen because you do not have a screen there. Take a small portion of the spectrum, and condense it further with a very small lens fixed in a blackened box, so as to get a very condensed beam. This is supposed to be a vessel containing one of these fluids, and the appearance you get is this, but so long as you are in the visible spectrum there is, generally speaking, an image of the double cone due to the light reflected from motes, which it is practically impossible to get rid of; that is followed at a certain interval by a beam extending over a greater or less width of the spectrum, and which is heterogeneous, that is

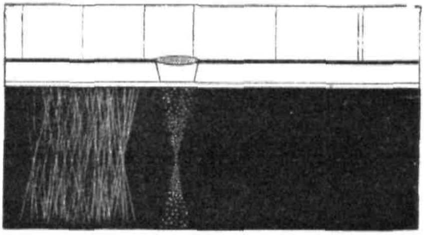

FIG. 2.

to say contains lights of various degrees of refrangibility. And if you analyse the light by a Nicol's prism or double-image prism, you will see that this speckled image is almost wholly polarised in the plane of reflection, indicating that it is merely due to reflected light, whereas the other, in the case of a solution, is wholly unpolarised.

The phenomenon being thus explained, so far as to make out the immediate nature of it, it was to be expected that something of the same kind would be observed in other instances of what Sir David Brewster called internal dispersion, but I may mention that under that term he classed together two phenomena which in reality are utterly different as to their nature. In certain cases you get what is virtually a powder in fine suspension, so fine that

it does not subside in any reasonable length of time, and gives you what looks like a pretty bright solution, but which really contains suspended matter. In such cases if you introduce a beam of condensed sunlight, the path of the beam is marked by light, because these motes reflect the light which falls upon them, but that light is reflected and is polarised by reflection, and this origin of the light is known by its polarisation. Moreover, being simply reflected it sends back the light which falls upon it unchanged in kind, whereas the truly dispersed light differs altogether in its nature from the light which falls upon the solution, glass, or crystal that shows it.

Now as I say the phenomenon being referred to the cause just explained in the one case, you may expect that in other cases something similar would be perceived, and I will take now the green solution obtained from leaves. I will suppose the experiment exactly the same, but the result is different in appearance although the nature of the phenomenon is the same. The path of the rays within the green fluid is marked by a blood-red light, which in different parts of the spectrum penetrates to a greater or less distance into the fluid. In this case the phenomenon begins very near the red end of the spectrum, somewhere about the line B of Fraunhofer. You first have a dart of red light extending right across the vessel. Then you come to a region of the spectrum for which the fluid is excessively opaque, and the red light, which is produced by the action of the incident light on the fluid in some way or other, cannot therefore extend very far into the fluid. Then you come to a region where the fluid although still opaque is comparatively transparent, and the red is traceable further inwards, and so on. It is noticed, however, after close observation, that where the incident rays are quickly used up the red light is very copious, and where they are more slowly used up the red light is not so strong.

In the figure some of the red bands are slightly shaded in the middle, to indicate that the red light is not so strong there as elsewhere. In the case of this fluid the effect is produced mainly by the visible spectrum, but it extends beyond that into the invisible region beyond the violet; so that the solution of quinine, and the alcoholic solution

of the green colouring matter of leaves may, to a certain extent, be regarded as extreme cases of the same general phenomenon.

I will refer now to this diagram. It consists of two parts; the upper part is diagrammatic for the same reasons as before; that is, you do not see the upper half and the under half simultaneously, but you may see first one and then the other. When the green colouring matter is purified by a particular process which would take too long to describe, and you take rather a dilute solution of it, and analyse the transmitted light, you get these bands of absorption. When you allow the upper spectrum to enter a very dilute alcoholic solution in this manner, then the red light which is given out in the regions of the spectrum near which the fluid is comparatively transparent, is given out there comparatively slowly, and accordingly it is less brilliant in the neighbourhood of the surface than is that corresponding to regions of great absorption. When the fluid is extremely dilute, then the intensity of the light in the former part of the spectrum becomes so small that you hardly see it, but its intensity where the fluid uses up the incident light more quickly in producing this phenomenon may still be visible, so that you may get corresponding to the position of these bands of absorption in the transmitted light these red bands where the light is given out very copiously, the solution being so dilute that the red light given out in the intervals is hardly perceptible at all.

These phenomena are not very easily shown at a distance to a large audience, but they are very striking when you look at them, as two or three at a time may, in a room where the sunlight is introduced or where an electric lamp is used. I will, however, endeavour by and by to show some of them by the aid of the electric light, but I am afraid they will be seen very feebly compared to what I have described, which are the effects seen when the light is concentrated into a small space.

Now allow me to go back for a moment to explain one particular matter I forgot at the time. I said that the light transmitted through a solution of sulphate of quinine was ordinary light, and that if you examined it and scrutinised especially the blue part of the spectrum, you would see nothing to account for the blue colour. If you

examine it by a prism applied to the eye, you will, however, see that the violet, or more or less of the violet, is gone; instead of seeing as is usual the double line H, you will see the fluid terminate, according to the strength of the solution and the thickness looked through, more or less towards the violet, say on an average about half way between the fixed lines G and H.

Now the incident rays work on the fluid in such a manner as to cause it to give out light of a different kind altogether; a light which is found to be heterogeneous, or to consist of rays of various degrees of refrangibility. This rule I find to be universal, namely, that the refrangibility of the light in this process is *always lowered*. I have never found any exception to that, nor I believe has anyone since.[1] The rays which any one of these fluids is capable of giving out under the influence of these other rays are always of lower refrangibility, and you never have the refrangibility raised.

I will endeavour presently to show a test tube with one of these solutions in part of the spectrum, though I cannot promise that it will be seen at a distance. The fact is I am accustomed to work with sunlight rather than with the electric light and I require more preliminary trials than I allowed myself for making the thing succeed. Still I think you see that on interposing a test-tube with the solution of quinine in the beam from the electric lamp, after it has passed through the prism, it cuts off certain portions of the spectrum thrown on the wall beyond, forming a shadow which shows in what part of the rays proceeding to form the spectrum the tube is for the moment placed; the blue light with which the solution glows, commencing about the violet, is seen altogether beyond the region of the visible rays. Here is a solution of a substance obtained from the bark of the horse-chestnut which shows it still better. You observe the blue band beyond the visible spectrum altogether. Another instance is when we allow the beam of light to fall on a piece of red cloth, it shows an orange band beyond the visible rays.

[1] Calorescence, or the exhibition of light by a body intensely heated by the concentration upon it of invisible heat-rays, is in some respects so different from the phenomena of fluorescence or phosphorescence that I do not regard it as forming any exception to the rule.

I have been a little anticipating what was to come, namely, that these phenomena are not confined to fluids or clear solids, but that they can be seen in every case. I have shown you that in a spectrum if you separate out the rays from one another by prismatic refraction you can see the phenomena in the invisible rays. But there is another mode of separating light into two portions of which one is allowed to pass, which is easier in practice, and which exhibits some of these phenomena very beautifully—that is by analysing it by absorption. For instance, I have here a very deep blue glass which cuts out most of the visible light, but it admits the violet and certain invisible rays beyond very copiously. It is a cobalt blue glass, and here is a yellow glass. I will analyse the light, not by a prism, but simply by absorption. This jar at present contains nothing but water. Now I will put the blue glass on to the electric lamp, and I have here a solution obtained from the bark of the horse chestnut, a little of which I will drop into the jar. If you make a decoction of the bark, which contains a good deal of tannin, the solution soon becomes brown. It contains however two crystallizable substances, called esculin and fraxin, which can be obtained chemically pure. The alkaline solutions of these bodies have this power in a high degree. You can get rid of the tannin by making a decoction, and when cold adding some persalt of iron or salt of alumina, precipitating by ammonia, and filtering, and you then get this beautiful solution, which will keep very fairly. I will pour some of this into the water, and as it sinks down it forms a beautiful blue cloud. If I hold the blue glass so as to intercept the incident rays hardly any diminution will be perceived, while the yellow glass cuts off most of the rays by which the effect is produced; but if I put the yellow glass between the jar and your eyes, a great deal of the light is transmitted The general effect is shown very well by means of these glasses. Here also are some jars containing some fluorescin and other fluids which can be tried in the same way. A coloured medium will absorb in a different manner the rays that fall on the fluid and the rays coming from it. That leads to one method of observation, which does not require the apparatus which I have hitherto supposed, but is exceedingly simple and at the same time very effective. You do not even require sunlight,

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you can work with ordinary daylight. Suppose you have a room which you can darken, and that you are at liberty to cut a hole four or five inches square in the shutter, under which it is convenient to screw on a ledge for the sake of supporting the object to be examined, and that you cover the hole by a suitable glass. The most useful generally is a dark blue coloured by cobalt, or a dark violet coloured by manganese. The blue does better for some things and the violet for others. Cover the hole in the shutter with the deep coloured glass or, as is occasionally better, with a solution of a salt of copper, such as the nitrate, which is more convenient than the sulphate on account of its great solubility. Suppose you have daylight filtered as it were through the deep blue or violet glass; then if you place in front of the glass a test tube containing a solution of quinine in dilute sulphuric acid, or a solution obtained from the bark of the horse-chestnut with a little ammonia, you will see this blue phosphorescent-looking light to perfection. But supposing you have not a window-shutter which you can make a hole in, still you can get on very well with an old packing-case. You knock off part of the top of it, so that you may look in, and saw off a portion obliquely parallel to the opposite top edge and on the slanting rectangular hole thus formed you nail a piece of board, making a window in it four or five inches square, with a little ledge to keep the glass with which you cover it from slipping down. You place it near the window and cover your head with a dark cloth as if you were looking into a camera obscura, and so you can see the phenomenon to perfection.

If you want to demonstrate that it is really this phenomenon you are dealing with, it is desirable to have a second glass in a certain sense complementary to the first. If we could pick out media which absorbed light just in the way we wished, we should choose a coloured glass perfectly opaque from the red end up to the blue or violet, and perfectly transparent beyond, and a second glass perfectly opaque for those rays for which the first was transparent, and *vice versâ*. But as we cannot make media to command to absorb what parts of the spectrum we like, we must make use of the best which the colouring matters of nature afford us; and if you take a blue glass and a yellow glass they will in

most cases answer the purpose sufficiently well. Suppose then you have a dark glass on a window-shutter and you have in front of it a substance to be examined, and it gives out this beautiful phosphorescent light. In general this may be at once distinguished from mere scattered light; but to make sure of it use your yellow glass, and place it between the blue and the substance you are examining. If it is well chosen it will cut off almost all the effect. Then place it between your eye and the medium which is shining with this phosphorescent light and you will see it quite plainly. The difference of effect with this additional glass in the two positions proves that you have really to deal with this peculiar phenomenon, and enables you to at once distinguish it from some appearances which at the first glance resemble it very much. If you put a minute quantity of a solution of proto-chloride of tin into a large quantity of common water, the mixture will have a bluish look by reflected light, and if you condense sunlight upon it you will get a beam somewhat like what you do when you receive a beam on the solution of sulphate of quinine. This however is merely scattered light; and that it is so is shown at once when you come to make experiments upon it in which you strain the incident light. For example, if you place the mixture inside your darkened chamber in lieu of the solution of quinine, the difference will appear in a moment. The mixture will merely give out a little light of a deep blue colour, which is scattered light, whereas the solution of quinine will be lighted up with this beautiful light that you see. I may mention a very simple and pretty experiment which can be made in that way. Take a bit of common horse-chestnut bark, float it in a glass of still water in which a drop of ammonia had been mixed; the peculiar substances contained in the bark will begin to be dissolved, the solution will descend, and you will see streams of descending blue light. If you can obtain specimens of these substances esculin and fraxin, a minute quantity of the two thrown together on the water instead of the bark looks very pretty. The substances will form little luminous specks here and there on the surface, which will give rise to descending streams of blue and greenish light.

Now there is another way of testing the change of

refrangibility almost without apparatus, by using your darkened chamber with a piece of blue glass in the window. Suppose that in front of the blue glass you place a piece of white earthenware, such as a saucer turned upside down. If you hold a slit at arm's length and view it through a prism, in the first instance aiming at the blue glass and looking up at the sky, you will see the sort of light transmitted. The brighter parts of the original spectrum will be almost entirely wanting, but you will see the violet, much of the blue, and the faint extreme red which is freely transmitted, and if the glass be not very deeply coloured a little faint greenish yellow which is not yet wholly absorbed. If you now aim at the white plate instead of the sky, you will see just the same spectrum as before, only not quite so strong. Now suppose you lay on the white plate a little bit of ordinary scarlet cloth, hold the slit close to that, and aim at both the cloth and the white plate, so as to get from different parts of the slit a spectrum of the light coming from each. This particular cloth in the blue field would look red. On examining the joint spectrum, the part seen by projection of the slit on the plate will appear as just described, while that seen by projection on the scarlet cloth will show a prolongation of the extreme red, and a great deal of bright light where there is none in the incident light, while the violet part will be nearly black.

This diagram, which was made for another purpose, may serve to exhibit what you would see in that particular case. It really represents what you see by looking through a crystal of nitrate of uranium placed immediately behind the slit with which you aim at the white light sifted through a blue medium. There are certain bands of absorption where there is a maximum of opacity in the incident light; and when you analyse the beam of light which comes through you see in the transmitted rays there are certain dark bands of absorption; but over and above that, there is light created with a refrangibility less than exists at all in the incident beam, and in the particular case of the salt of uranium the prismatic composition of this light is very peculiar, its spectrum consisting of bright bands.

Now as I want to show you how to make experiments yourselves without apparatus, I may mention, that supposing you have one of these solutions in a test tube, you very

much increase the effect by plunging the test tube in water and looking at it downwards nearly parallel to the test-tube. The reason of that is that the incident rays fall upon the fluid and cause it to give out light in all directions, blue light, or whatever else it may be, according to the nature of the fluid; but when the water is not there, a portion of the light so given out does not enter the eye at all, but suffers total internal reflection at the outer surface of the tube. On the other hand, if you look inside the tube the light suffers absorption on the part of the fluid itself. It does not much signify in the case of sulphate of quinine, which is sensibly transparent, but if you examine any coloured fluorescent fluid looking down the tube inside, you lose light from defect of transparency, the light being absorbed by the fluid; whilst if you look outside you lose a great deal by the total internal reflection at the surface of the glass. But if you plunge the test-tube into water, and look down from the outside, any of the emitted light which gets into the glass of the tube is able to get out again, so that you can look down from above in a very slanting direction and still get all the light, and as the stratum which emits the light is seen very much foreshortened, the brightness of the light is thereby increased.

Now as to the cause of this phenomenon. From the first I believed the cause to be this: that the incident rays so act on the ultimate molecules of the body as to throw them into a state of agitation, which agitation they in their turn are capable of communicating to the ether. Everybody now, I believe, considers that light is produced by the vibration of a certain subtle medium, which we call the luminiferous ether, and I will take a dynamical illustration of the phenomenon according to this view. Suppose you had a number of ships at rest on an ocean perfectly calm. Supposing now a series of waves, without any wind, were propagated from a storm at a distance along the ocean, they would agitate the ships, which would move backwards and forwards; but the time of swing of the ship would depend on the time of its natural oscillation, and would not necessarily synchronise with the periodic time of the waves which agitated the ship in the first instance. The ship, being thus thrown into a state of agitation, would itself become a centre of agitation, and would produce waves

which would be propagated from it in all directions. This I conceive to be a rough dynamical illustration of what takes place in this actual phenomenon, namely, that the incidence of etherial waves causes a certain agitation in the ultimate molecules of the body, and causes them to be in their turn centres of agitation to the ether; in fact that the incident light renders the medium so to speak self-luminous, so long as it is under the excitement of the incident light. That is the view which I maintained from the first, and which is clearly expressed in my original memoir, which was published in the *Philosophical Transactions* of 1852. There is one phenomenon, that of phosphorescence, which I felt from the first to be exceedingly analogous to that which is now known by the name of *fluorescence*, a word I suggested in that original memoir, derived from fluor spar, which was one of the first minerals in which the phenomenon had been observed, as the analogous term, opalescence, is derived from the name of the mineral opal. I am unable to draw any sharp line of demarcation between fluorescence and phosphorescence. So far as I had observed, the effect was only of instantaneous duration, although, as I have expressly stated, I had not made experiments on a revolving mirror to determine whether a finite duration could be perceived With regard to the explanation of the law which I believed to be universal, that in this phenomenon the refrangibility is always lowered, that is to say, the light coming out is always of lower refrangibility than the incident light, I offered a certain conjecture, which I did not hold to very tightly, and I have somewhat changed my views in that respect; but I held from the first that the effect is not a direct but an indirect one; that the light is not simply reflected from the ultimate particles of bodies. It is curious that some two or three writers have attributed to me the notion that in this phenomenon the light reflected from the molecules of the body was changed in refrangibility. They have attributed that notion to me and then contended against it; but if you will allow me to read a short passage from my original paper, it will show that I am not responsible for that. I wrote these words: "In considering the cause of internal dispersion, we may, I think, at once discard all supposition of reflections and refractions of the vibrations

of the luminiferous ether amongst the ultimate molecules of bodies. It seems to me quite contrary to dynamical principles to suppose that any such causes should be adequate to account for the production of vibrations of one period from vibrations of another." Having written that, I am not responsible for the view which has been so wrongly attributed to me. I can only account for it in this way. I suppose it was from the title I gave my paper, "On the change of refrangibility of light," which I chose because undoubtedly the most striking part of the phenomenon, which had not been hitherto suspected, was that the light given out was of a different refrangibility from the light going in. With regard to the duration of the phenomenon, I thought it *possible* that, though very large compared with the time of a single luminous vibration, it might elude our means of observation; but subsequently Mons. E. Becquerel showed, by a very ingenious instrument, which is here on the table, which he calls a phosphoroscope, that the phenomenon is really of appreciable duration. The light enters at one side, and falls on a cell containing the substance to be examined, in this case a salt of uranium enclosed between a pair of discs constituting a double fly with holes of a somewhat sectorial shape, so arranged that the holes in the one exactly correspond with the spaces between the holes in the other. When you turn the wheel round, supposing there is no substance there, you see no light, because you are always looking across a plate of metal. At one time there is a hole in the front disc, which is covered by the second plate, and when there is a hole in the second plate it is covered by the front disc; a series of holes coming alternately in the two discs. But supposing a substance is interposed, and the light is let in, and you turn the wheel, the illumination is let on and cut off alternately with great rapidity; but you see the light, not at the moment when the body is illuminated, but a small fraction of a second afterwards, through the hole which is opposite your eye; in that way you see it a very short space of time after it has been lit, so to speak, by the incident light; it gets a number of doses of light in one rotation, and you get a number of glimpses of it immediately afterwards. In that way many substances which show this phenomenon appear luminous, and you see them by the light

which has been treated in this manner. I may mention also that even a simple revolving mirror will show the duration of the effect in such cases as the salts of uranium and solids in general. If you use an ordinary electric machine as a source of illumination, giving a succession of sparks, or an induction coil with a Leyden jar in connection with the two terminals, which gives a momentary discharge, you can observe the substance in a rapidly revolving mirror, and in that way you get a momentary view of the substance by reflected light, while the illumination due to fluorescence, in case it has an appreciable duration, is drawn out into a broad gleam; so that even without an instrument of the kind now on the table the duration of the effect can be manifested by experiment. The duration of the effect, I may observe, has not as yet in any instance been demonstrated experimentally in the case of a liquid.

When epipolic dispersion was referred to a change of refrangibility of light, there were some older experiments which at once received their explanation. For instance, Sir John Herschel himself, on throwing a pure spectrum on turmeric paper, had noticed a great prolongation of the ordinary visible spectrum. But he supposed that this was due to the ultra violet rays, which were directly reflected into the eye; and he speculated whether there might not be a repetition of the colours of the ordinary spectrum. This, however, proved to be a phenomenon of the kind I have just described, fluorescence, or, if you like so to call it, phosphorescence, as I am persuaded that fluorescence is nothing but phosphorescence of brief duration. Mons. E. Becquerel went still nearer to the actual phenomenon, for he was making experiments on substances which show phosphorescence after exposure to light, and observed that some of them were specially luminous when light fell upon them and was acting upon them Nevertheless, although he correctly explained what he witnessed in these cases, from connecting it too closely with phosphorescence, he failed to perceive the full bearing of his own observation; and though he had actually under his hands the solution of quinine, and had discovered by means of photography the intense absorbing action of that fluid on the invisible rays, and expressly mentioned the "dichroism" of the solution he never dreamt of putting the two things together.

and showing that the peculiar coloured light exhibited in this and other allied instances, which were matters of *ordinary observation,*[1] *had an origin hitherto unsuspected*

I have here some phosphorescent tubes which have been lent to me, which you will see retain the luminosity for some time in the dark after the light of the electric lamp has been allowed to play upon them. The most phosphorescent substances are certain sulphides of metals of alkaline earths, though it is said that certain impurities contribute to the effect rather than otherwise.

In the course of my experiments I was led to see that glass was by no means transparent in regard to the most refrangible of the rays I had to deal with. I procured accordingly prisms and lenses of quartz, with which to form a pure spectrum. On applying them to the solar spectrum, *the invisible rays were seen to extend far beyond anything* I had ever seen before, and showed a continuation of Fraunhofer's lines that is represented on these maps, which were originally drawn for an evening lecture I gave on the subject for the British Association at Belfast in 1852. In the spring of the next year, while preparing for a lecture at the Royal Institution, in the laboratory of that institution, along with Faraday, though I had expected beforehand to obtain a very long spectrum by the use of the electric light, I was utterly surprised when I found the actual length of it. I used in the first instance a Leyden jar, and I could not but think at the first moment that there was some stray reflection of light, for if the visible spectrum was about one inch, the whole spectrum obtained by means of powerfully fluorescent substances, such as uranium glass, with the electric light, was about six or eight inches, which was a length I had not been at all prepared for.

[1] The very name "Schillerstoff," formerly given to æsculin, is derived from the property in question.

THE KINEMATICS OF MACHINERY.

ILLUSTRATED BY THE BERLIN COLLECTION OF KINEMATIC MODELS.

TWO LECTURES.

BY PROF. ALEX. B. W. KENNEDY, C.E., OF UNIVERSITY COLLEGE, LONDON.

The substance of the following pamphlet formed two lectures delivered to Science Teachers at South Kensington during the month of August 1876. *They have been recast only so much as to make them intelligible without the aid of the set of models by which they were then illustrated. Any readers who may wish further to study Reuleaux's treatment of the subject, of which I have attempted here to note a few salient points, I must refer to his own "Theoretische Kinematik," or to my translation of it, published under the title of "The Kinematics of Machinery." In this work the whole matter is taken up in great detail from the point of view which I have endeavoured to indicate.*

A. B. W. K.

LECTURE I.

Most of the models used to illustrate this and the following lecture belong to the Kinematic Collection of the Gewerbe-Akademie in Berlin, and have been designed by Professor Reuleaux, who is the Director of the Academy and a Professor in it. The rest were sent to the Loan Collection by Messrs. Hoff and Voigt of Berlin, and Messrs. Bock and Handrick of Dresden. In essentials there is no difference between the Berlin and the Dresden models. Both have been designed specially for use in instruction in the Kinematics of machinery.

I must first try to explain briefly but exactly what I mean by the phrase "Kinematics of machinery." Professor Reuleaux, whose models are before us, defines a machine as "a combination of resistant bodies so arranged that by their means the mechanical forces of nature can be compelled to do work accompanied by certain determinate motions." The complete course of machine instruction followed in some of the continental technical schools covers something like the following ground:—

First, there is the perfectly general study of machinery,

technologically and teleologically. Then there comes what we may call the study of prime-movers, which in terms of our definition would be the study of *the arrangements by means of which the natural forces can be best compelled to do the required work.* Then comes the study of what may be called "direct-actors," or the direct-acting parts of machinery; in the terms of our definition, *the arrangement of the parts of a machine in such a way as best to obtain the required result.* Next comes what we call machine design; the giving to the bodies forming the machine the requisite quality of resistance. Machine design is based principally on a study of the strength of materials.

One clause of the definition still remains untouched. The machine, we said, does work *accompanied by certain determinate motions.* Corresponding to this we have in machine instruction the *study of those arrangements in the machine by which the mutual motions of its parts, considered as changes of position only, are determined.* The limitation here must be remembered; motion is considered only as change of position, not taking into account either force or velocity. This is what Professor Willis long ago called the "science of pure mechani-m," what Rankine has called the "geometry of machinery," what Reuleaux calls "kinematics," and what I mean now by the "kinematics of machinery."

The results of many years' work of Reuleaux in connection with this subject are embodied in his book *Die Theoretische Kinematik*, which I recently had the pleasure of translating, and I shall endeavour to give you an outline of his treatment of the subject. It cannot be more than an outline, as you will readily understand. The subject is a very large one, and I have had to choose between taking up many branches of it and merely mentioning each, and confining myself to a few points, and going more into detail about them. I have chosen the latter plan, believing that the former would be of little benefit to anybody. It will be easy for those who are sufficiently interested in the matter to follow it up, and to study those parts which I omit by the aid of the book I have just mentioned. My lecture to-day will be principally theoretical, and to-morrow I shall go more into practical applications. So far as possible, as I have Professor Reuleaux's models before me, I shall endeavour to follow his own order in treating the subject.

I presume you are acquainted to a certain extent with the ordinary method of studying "pure mechanism;"—the method originated by Monge (1806), developed in Willis' well-known *Principles of Mechanism* (1841) and made popular to a great extent by Prof. Goodeve's capital little text-book and others. Each mechanism is studied for and by itself, in general by the aid of simple algebraic or trigonometric methods, and is spoken of in reference to a certain "conversion" of motion which occurs in it. Thus we have the conversion of circular into reciprocating motion, the conversion of reciprocating into circular, &c., and simple formulæ express certain relations between the motions of two or more moving points. In this way we know something important about a great number of mechanisms, and arrive at many results which are both useful and interesting. Some things are still left wanting, however; and these things may be summed up in this way:—

(1) We notice at once that we have taken the mechanism as a whole. We do not *analyse* it in any way whatever, and therefore,

(2) We have scarcely any knowledge of its relations with other mechanisms, or (what is quite as important) of the various forms which one and the same mechanism may take. We shall see presently how extraordinarily various these forms are. We have never a *general* case with special cases derived from it; each case is treated by itself as a special one. Then

(3) The mechanism is studied in general from a point of view which gives us only the conditions of the motion of two points in it, or two portions of it, and is then left. The kinematic conditions of the mechanism *as a whole* remain absolutely untouched.

In such a mechanism as that of an ordinary steam-engine, for instance, we study the relative motions of the guide-block and the crank, or I ought perhaps to say of the axes of the crosshead and of the crank-pin. We thus know the motions of two points in the rod which connects those axes, the "connecting-rod," but we leave the motions of all its other points untouched. It may, of course, be said that these others are of much less practical importance. This is true to some extent, although their practical importance is greater than might be supposed at first. But in any case

these motions must certainly be studied if we are to obtain a *complete* knowledge of the mechanism to which they belong. Any method of study, therefore, which covers all the kinematic conditions of the mechanism, instead of the mechanical conditions of two or three points only, possesses in that respect very great advantages.

The treatment of mechanisms which I shall sketch to you is intended to remedy some of the defects which I have enumerated. Those of you who have studied modern geometry side by side with the old methods will recognise that these defects are somewhat analogous to those of Euclidean geometry. The attempt to remedy them proceeds in lines similar to those of modern geometry, and will eventually, I believe, when more fully worked out, take the same position in its own subject.

Let us then look first at the *analysis of mechanisms.* This is none the less important a matter that its results are so very simple in many cases. A clear understanding of these elementary matters is of great assistance in clearing up difficulties which occur in the more advanced parts of the subject.

In a machine or a mechanism of any kind *the motion of every piece must be absolutely determinate at every instant.* It will be remembered that we are at present considering motion as *change of position* only, not in reference to *velocity.* The motion or change of position *may* be determined by the direction and magnitude of all the external forces which act on the body: the motion is then said to be *free*, but it is obviously impossible to arrange such a condition of things in a machine. The motions may, however, be made absolutely determinate independently of the direction and magnitude of external forces, and in order that this may be the case the moving bodies, or the moving and fixed bodies as the case may be, must be connected by *suitable geometric forms.* Motion under these circumstances is called *constrained* motion.[1]

If I allow a prismatic block to slide down the surface of an inclined plane its motion will be free; it is determined by the combination of external forces which act upon the

[1] Essentially it does not differ from free motion; the difference really lies in the substitution of *stresses* or *molecular forces*, which are under our complete control, for external forces.

block. If the block be pressed on one side as it slides, it at once moves sideways, and can only be kept in a straight path if directly the pressure is exerted on the one side an equal and opposite force (or a force which has a resultant with the first in the direction of motion), be caused to act upon it on the other. If, on the other hand, the block be made to slide between accurately fitting grooves (like a guide-block in a machine), inclined at the same angle as the plane, and like it fixed, the block may be pressed sideways or in any other direction, but no alteration in its motion can take place; the motion is "constrained," it can occur only in the one direction permitted by the guiding grooves. In the one case the external force has to be balanced by another external force, in the other the balancing force is molecular, *i.e.* is a *stress* and not an external force, and comes at once into play the instant the disturbing force is exerted. The geometric forms which are used in this way to constrain or render determinate the motions in machines are very various, and are chosen in reference to the particular motion required. If every point in a body be required to move in a circle about some fixed axis, a portion of the body is made in the form of a solid of revolution about that axis, and this is caused to "work in" another similar solid; the two forming the familiar pin and eye. If all points of a body be required to move in parallel straight lines we get similarly for guiding forms a pair of prisms of arbitrary cross-section; a slot and block. If every point of a body be required to move in a helix of the same pitch we use a pair of screws of that pitch, one solid and one open, for constraining the motion;—a screw and nut.

The *general* condition common to these very simple forms is that in each case *the path of every point in the moving body is absolutely determined at every instant,* that is to say, the change of position of the moving body is absolutely determinate.

The geometric name for these mutually constraining bodies is *envelopes,* and each one is said to envelope the other. We shall call them (kinematic) *elements,* and the combination of two of them we shall call *a pair of elements.*

Those we have mentioned are special and very familiar and important cases of pairs of elements, which are of great simplicity. They have the common property of surface

contact, the one enclosing the other, and are therefore called *closed* or lower pairs of elements. They are, moreover, the only closed pairs which exist. They are further the only pairs in which all points of the moving element have *similar* paths.

Every point of an eye, for instance, moves in a circle about the same axis. If there were attached to it a body of any size or form whatever, all its point would move about the same axis. The "point-paths" would all be concentric circles. Again, whatever the external size or shape of a nut, every point in it moves in a helix of the same pitch about the axis of the screw; the point-paths, that is, would be similar.

The general condition of determinateness of motion can, however, be fulfilled by an immense number of other pairs of elements. The theory of these is too large a subject to be entered into just now, I must merely direct your attention to the existence of such combinations.

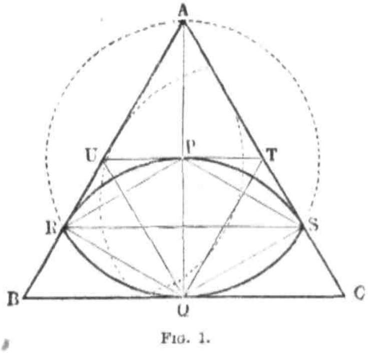


FIG. 1.

Fig. 1 represents one of the simplest that can be used. Here one of the elements is an equilateral triangle, *A B C*, the other is the "duangle" *R P S Q*. The latter moves within the former, touching it always in three points, or rather along three lines. Its motion is just as absolutely determinate as the motion of a pin in an eye. It is free to move at any instant only about the point in which the three normals to the triangle at the points of contact intersect (as *Q* in the Fig.) The models before you show a few of the many forms

taken by such pairs of elements. It is worth while noticing a few points in which the motions determined by them differs from the motions of the closed pairs. First, as we have already seen, the contact of the elements determining the motion was surface-contact in the former case, while here it takes place only along a finite number of lines. Then the motions of all points in the first case were similar; in these pairs the motions of the points are not similar, but entirely

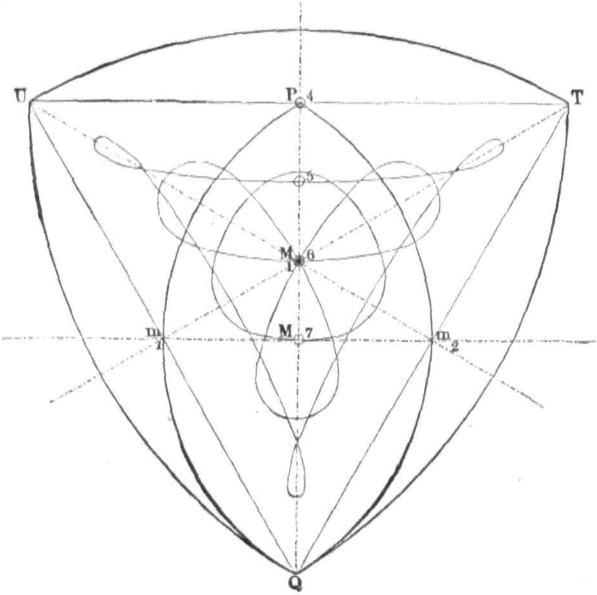


FIG. 2.

dissimilar, the motion of each point depending entirely upon its position. Fig. 2 shows a few of the point-paths of the pair of elements shown in Fig. 1. The strikingly different curves obtained from one pair of elements, according to the choice of the describing point, is too obvious to need further notice.[1]

[1] The triangle $U\,T\,Q$ and the three curves within it, which have M_1 for their centre, are point-paths. The curve-triangle and the duangle shown in thicker lines will be explained further on.

These pair of elements are called *higher pairs*. They have only a few applications in practice, their interest being chiefly theoretical. From our present point of view their theoretic interest is considerable, because of their exact analogy with the lower pairs.

There is another difference between the two kinds of pairs which deserves notice, for reasons which will be better understood afterwards. The pair of elements determines the relative motion of the two bodies connected by it. If one body be stationary on the floor or the earth, the moving body has the same motion relatively to the floor or earth that it has to the other element. If I move about both bodies in my hand, both have motion relatively to the earth, but the relative motion of the one to the other remains unchanged. It is of course only a case differing in *degree* from the former one, for in the former one both bodies had the motion of the earth itself, while one had the additional motion which I gave it. We may, however, not to be pedantic, speak of anything as "fixed," or "stationary" which has the same motion as the earth.

Now (in this sense) we may *fix* either element of a pair, and with the lower pairs the *relative motion taking place remains the same* whichever element be fixed. With the higher pairs, on the other hand, the relative motion is altered, and the point-paths become entirely different. The point-paths of the duangle relatively to the triangle are, for instance, quite different from those of the triangle relatively to the duangle. This change of the fixed element is called the *inversion* of a pair.

The ultimate result of our analysis of mechanisms is then pairs of elements; we cannot go below this. The pairs we have noticed are of two kinds, each having their own definite characteristics. If now two or more elements of as many different pairs be joined together we get a combination which is called a (kinematic) *link*. It is obvious that the form of such a link is, kinematically, absolutely indifferent. The choice of its form and material belongs to machine-design. It may be brick and mortar, cast-iron, timber, as we shall see afterwards, but the fact that this is indifferent kinematically cannot be too distinctly kept in mind.

We can make combinations of links by pairing the elements which each contain to partner elements in other links, and such combinations are called *kinematic chains*. Thus if we denote similar elements by similar letters, *aa*, *bb*, *cc*, &c. and

G 2

the link connection by a line, we may indicate some of the chains obtainable from 4 pairs and 4 links, thus :—

a —— *bb* ——*cc* —— *dd* —— *a*

(we suppose the "chain" to return on itself and the two elements *a* to be paired, the whole forming a *closed chain*) ; or,

a —— *cc* —— *bb* —— *dd* —— *a*

or

a —— *dd* —— *cc* —— *bb* —— *a &c.*

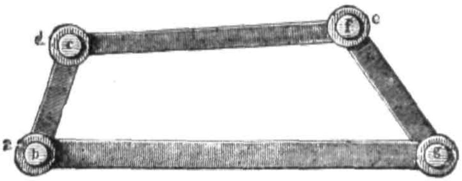


FIG. 3.

For the sake of illustration we give in Fig. 3 a sketch of a familiar chain containing four links, each connected to the adjacent link by a cylinder-pair of elements. The axes of the four pairs of elements are parallel.

We have then, in the kinematic chain a combination so constructed that all its parts have determinate motions, motions absolutely fixed by the form of the elements carried by its links, and independent (considered as changes of position) of the application of external force. To convert the chain into a *mechanism* we have only to do what we have already done in connection with pairs of elements, fix one element,—or, as each element is rigidly connected with a link, we may say preferably *fix one link.* Any link may be fixed, the chain therefore gives us as many mechanisms as it has links. In general these are different, in special cases only two or more of them are the same. We shall be able to enter into this part of our subject at some length in the next lecture ; at present it will suffice to note two or three of the leading characteristics of chains and mechanisms which we can now easily recognise. These are

(i.) That the motion of any link relative to either adjacent link is determined by the pair of elements connecting them.

(ii.) That the motion of any link relative to any other than its adjacent links depends on *all* the elements of the chain.

(iii.) That no one link of a *mechanism* can be moved without moving all the other links except the fixed one, and

(iv.) That there can be only *one* fixed link in a mechanism.

The two last propositions require a few words of explanation. Suppose that in any combination of, say, four links, two can be moved without moving the other two, the combination is actually one of three links only, for clearly the two immovable links may be made into one, and are two only in name. This is very often the case in machinery, where special mechanisms are frequently used for the express purpose of connecting rigidly two or more links, and making them act as one, at certain intervals.

If, however, in the combination supposed, one link be fixed, while two can be moved and the fourth can *either* move or be stationary, the combination no longer comes under our definition of *constrainment*, for the motions are at a certain point indeterminate, at the point, namely, when it is possible for the fourth link either to move or to stand. Chains often occur in which this would be the case were it not that mechanicians take means, either by adding other chains or in other ways, to constrain the motion which would otherwise be useless to them.

We have now obtained some idea of the way in which mechanisms are formed, of the elements of which they consist. Before applying the knowledge we have thus acquired I must direct your attention to some geometric propositions which will greatly facilitate the theoretic dealing with these mechanisms.

In order that I may not enter into too wide a subject, I shall confine myself here to the consideration only of "conplane" motions, or motions in which all points of the moving body move in the same plane or in parallel planes. The limitation is a large one, but the cases included under conplane motion cover the greater part of those which occur in practice. The method I have to describe is equally applicable to general motion in space as to simple constrained conplane motions of which I shall speak.

Let me remind you that the motion of any *figure* moving

in a plane is known if the motion of any two points (*i.e.* of a line) in it be known. The motion of any *body* having conplane motion is known if the motion of a plane section of it, parallel to the plane of motion, be known. Such a plane section of it is, of course, simply a plane figure moving in its own plane. The motion of any *body* having conplane motion (as in nine cases out of ten in machinery), can therefore be determined by the determination of the motion of two points. In speaking now, therefore, of the motion of a *line* for shortness' sake it must be remembered that we are really covering all cases of conplane motion of *solid bodies.*

In Fig. 4 PQ and P_1Q_1 are two positions of the same plane figure, or plane section of a body having conplane motion. If now we have two positions (in the same plane) of any plane figure, we know that the figure can always be moved from the one to the other by turning about some point in the plane. The position of the point O, about which the figure can be turned from the position PQ to the position P_1Q_1 can be found at once by the intersection of the normal bisectors to PP_1 and QQ_1. The motion of PQ in the plane is, of course, its motion relatively to the plane, and therefore relatively to any figure (as $A\ B$) in the plane. Such a point O as we have found here is called a *temporary centre,* because the turning or motion takes place about it for some finite interval of time. It will be remembered that not only the two points and $P\ Q$ of the figure, but every other point of it, must have a movement about this same point O at the same time. Now suppose we have some further position of the same figure, as for example at the position marked P_2Q_2, we can find in the same way the centre about which the figure must be turned to move from P_1Q_1 to P_2Q_2. We may indicate this point as O_1. Similarly taking other positions of this figure P_3Q_3 and so on, we can find other points, O_2O_3, &c. By joining the points $OO_1O_2O_3$, we obtain a polygon, and if the figure in its motion come back to its original position the polygon also comes back on itself, and passes again through the point O. Such a polygon, whether it be closed in this way or not, is called a *central polygon;* its corners are the temporary centres of the motion of the figure.

I have pointed out that all the points in the figure PQ move round O during the motion from PQ to P_1Q_1. They move round O necessarily through some particular angle, the angle

POP_1, and every point moves through the same angle, which we may call ϕ_1. As the figure may have any form we choose, let us suppose it so extended as to contain a line which is the same length as OO_1, and which makes with OO_1 the angle ϕ_1, that is to say, the angle through which the figure moves about O. Such a line is shown in Fig. 4 by MM_1.

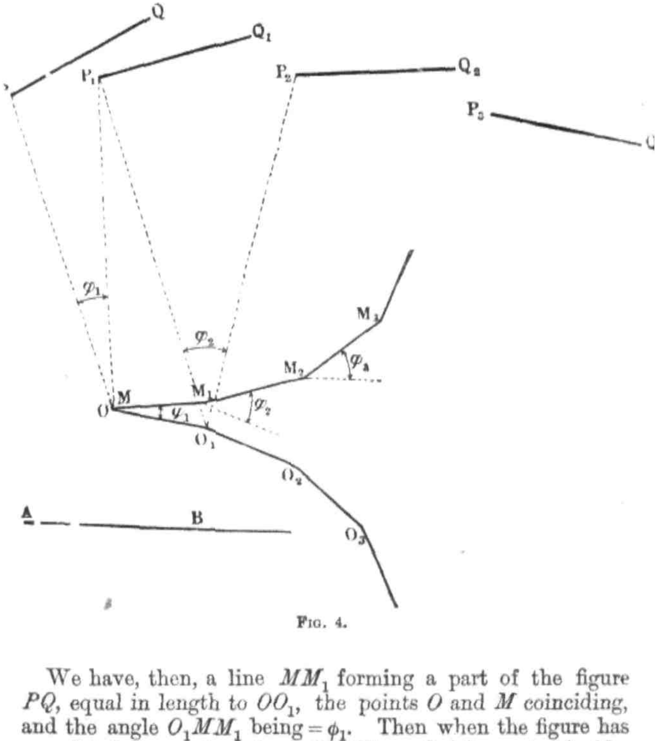


FIG. 4.

We have, then, a line MM_1 forming a part of the figure PQ, equal in length to OO_1, the points O and M coinciding, and the angle O_1MM_1 being $= \phi_1$. Then when the figure has completed its motion about O, MM_1 and OO_1 must coincide. Take further similarly $M_1M_2 = O_1O_2$ and so placed that when M_1 coincides with O_1, $\angle O_2M_1M_2 = \phi_2$, then when the figure takes its third position, completing the turning about O_1, M_1M_2 coincides with O_1O_2. Similarly we can obtain M_3M_4, &c.

The figure thus found is another polygon, which we may call a *second central polygon.*

These polygons have important properties, the principal of which can be very easily recognized. The first polygon does not alter its position during the motion of the body; it is therefore fixed, so that it may be considered *as a part of any figure such as AB* which is fixed or stationary in the plane of motion. The second polygon moves with *PQ and forms* (by construction) *part of the same figure with PQ.* This second polygon then, by the consecutive turnings of its corners upon the corresponding corners of the first (and equal-sided) polygon, will give to *PQ* the required changes of position relatively to the fixed plane or to the figure *AB* lying in it.

If therefore we know the central polygons for the given motion, we know not only the changes of position of the points *P* and *Q*, but those of every other point connected with the moving figure, whatever form it may have. For at any one instant every point in the figure is moving about the same centre. In studying the relative motions of the figures we may, therefore, quite leave out of sight their *form* if we only know the central polygons for the motion. These tell us, so far, all about the motion which is taking place.

We may go further, however. We have recognised the fact that the relative motion of two figures or bodies may take place equally whether one or the other of them be fixed, or both moving. In the case before us we have supposed *AB* fixed and *PQ* moving relatively to it. The second polygon then moves on the first, and expresses the relative motion taking place. If, however, we suppose *PQ* fixed and *AB* moving, then the polygons still express the relative motion; but the second is now fixed and the first rolls upon it. This follows directly from the constitution of the polygons. The properties of the polygons as expressing the relative motions of the bodies to which they belong are therefore reciprocal.

You will have noticed, no doubt, that the polygons do not express *continuous motion.* They define only a series of changes of position in their beginning and end, not telling us of the intermediate stages.

We may, however, take the consecutive position of the figures *as close together as we like.* The closer together they are taken the shorter become the sides of the polygons. If

at last the distances PP_1, P_1P_2, QQ_1, &c., be taken *infinitely small*, each corner of the polygon will be *infinitely close* to the next one. That is to say the two polygons will become *curves*, and of these curves "infinitely small parts of equal length continually fall together after infinitely small rotations about their end points." In other words the two curves *roll* on one another during the continuous alterations in the relative position of the two figures. Instead of finding points now by the intersection of normal bisectors, they are found by intersection of *normals to the paths* of P and Q

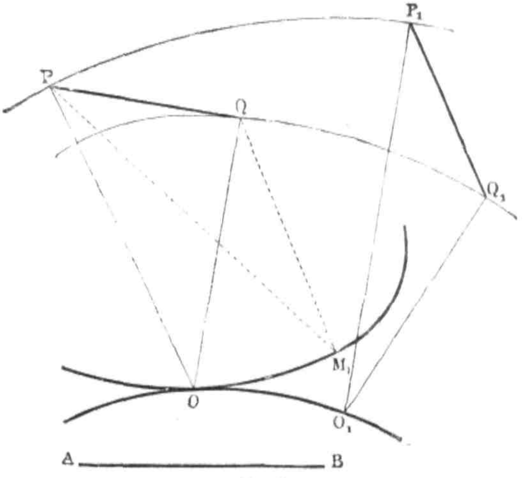


Fig. 5.

(Fig. 5). The turning about each point now occurs not (in general) for a finite period, but for *an instant* only. Each point is therefore called an *instantaneous centre*. The curve containing all the instantaneous centres, or the *locus* of instantaneous centres, is called a *centroid*. Without giving them any special name, several writers on Mechanics have made more or less use of these curves. Among these I may mention Dwelshauvers-Dery, Schell and Pröll. Reuleaux has, however, given them a name (*Polbahnen*), and has made

some special use of them, more, I think, than has been made by former writers.

While the polygons only represent a series of isolated positions of a body, the centroids, rolling on each other, represent the whole motion continuously. Like the central polygons their properties are reciprocal. If then the centroids of two figures be known, their relative motions for a series of changes of position, each infinitely small, are also known, *i.e.* their motions are *completely* determined.

If $A\,B$ and its centroid be fixed, and the centroid of $P\,Q$ rolled upon it (Fig. 5), we have now the means of determining the path of motion of every point in the Fig. $P\,Q$ relative to $A\,B$, whatever may be the form of $P\,Q$. It is

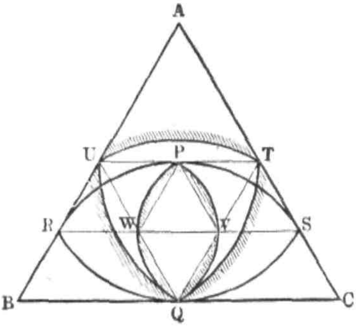


FIG. 6.

sometimes of great convenience to be able to find the motions of all points in a body in such a very simple way. Reciprocally we can determine the point paths of $A\,B$ relatively to $P\,Q$, which, in general, differ entirely from those of $P\,Q$ relatively to $A\,B$.

If *both* figures be moving, as frequently happens in practice, both centroids are also in motion; their motion relative to each other, however, remains unaltered. They still roll on one another, and their point of contact is still the instantaneous centre of the motion of each relatively to the other. Each figure moves, relatively to the other, about this point, which, being common to the two centroids, is common to the two figures. They might, therefore, for the instant, be

connected a that point by a cylindric pair of elements. There are many problems of which the solution is greatly simplified by the recollection of this fact. The point in each figure which coincides with the instantaneous centre, has, therefore, no motion relatively to the other figure. We have already seen this in the special case where the one figure is stationary, for then the point in which the moving centroid touches the fixed one is, by hypothesis, also stationary for the instant; in other words, it has no motion relatively to the fixed centroid. We now see the general condition of which this is a special case.

Fig. 6 shows the centroids for the higher pair of elements of Fig. 1. The curve-triangle *U T Q* is the centroid of the

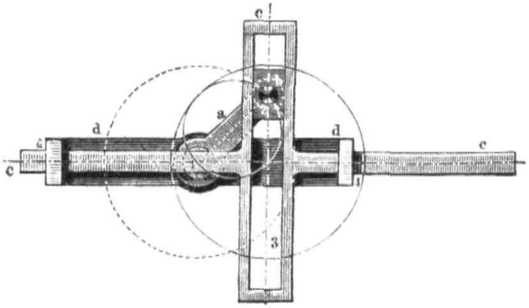


FIG. 7.

triangle *A B C*, and the shaded duangle *P V Q W* is the centroid of the duangle *R P S Q*.[1] As the duangle moves in the triangle (the elements *sliding* upon each other), its centroid *rolls* within the centroid of the triangle. Both centroids are in this case formed of arcs of circles, and all the point-paths (being determined by the rolling of one circular arc upon another) are combinations of trochoidal arcs.

The centroids of kinematic chains are generally of greater complexity that those of the pairs of elements just mentioned,

[1] These centroids are shown on a larger scale, apart from the elements to which they belong, in Fig. 2.

but in some cases are quite as simple. In Fig. 7, for example, is shown a mechanism familiar to engineers, in which a crank *a* drives a reciprocating bar *c* by means of a block *b* working in a slot. The centroids defining the relative motions of the links *a* and *c* are the two circles shown in full lines, one double the diameter of the other. These two circles both move as the mechanism works (supposing the link *d* to be fixed), but always so that they roll continuously one on the other. If instead of fixing *d* the crank *a* were made the fixed link, the same centroids would still express the relative motions of *a* and *c*. The smaller circle, the centroid of *a*, would be stationary along with the link to which it belongs, and the other would roll on it, the instantaneous centre for the motion of the link *c* being always at their points of contact. This mechanism (*a* being fixed) is used in Oldham's coupling, in elliptic chucks, &c. Knowing these centroids we know all about the motions of the two corresponding links in the mechanism, not only about the motions of some particular points in these links.

The centroids of kinematic chains can in general be very easily determined. Once found they make us independent to a great extent of trigonometric or algebraic formulæ, and enable us to determine all we wish to know by purely geometric graphic constructions. For technical purposes at least this is frequently an immense advantage. There are very few cases in which it is not more convenient for the engineer to employ a construction than a formula, if both give him the same result.

Before looking at the centroids of other mechanisms it is necessary to examine one particular case which often occurs. Suppose that the lines $P\ P_1$ and $Q\ Q_1$ in Fig. 4, or the tangents to the curves at P and Q in Fig. 5, had been parallel. It is obvious that the normal bisectors in the one case and the normals to the curve in the other then become also parallel, or, as it is for some reasons more convenient to express it, would meet at an infinite distance. The temporary centre in the one case and the instantaneous centre in the other are at infinity. A centroid may therefore contain one or more points at an infinite distance, may have, that is, one or more infinite branches. This constantly occurs in mechanism, and in some cases *every* point in the centroid is at an infinite distance. This is however a special case; its treatment does not offer

any practical difficulty, but I cannot do more than mention its existence here.

The centroids of the connecting rod and frame of the ordinary steam-engine driving mechanism (the links *b* and *d*

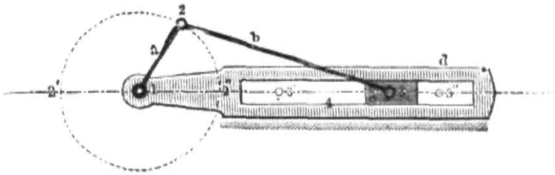


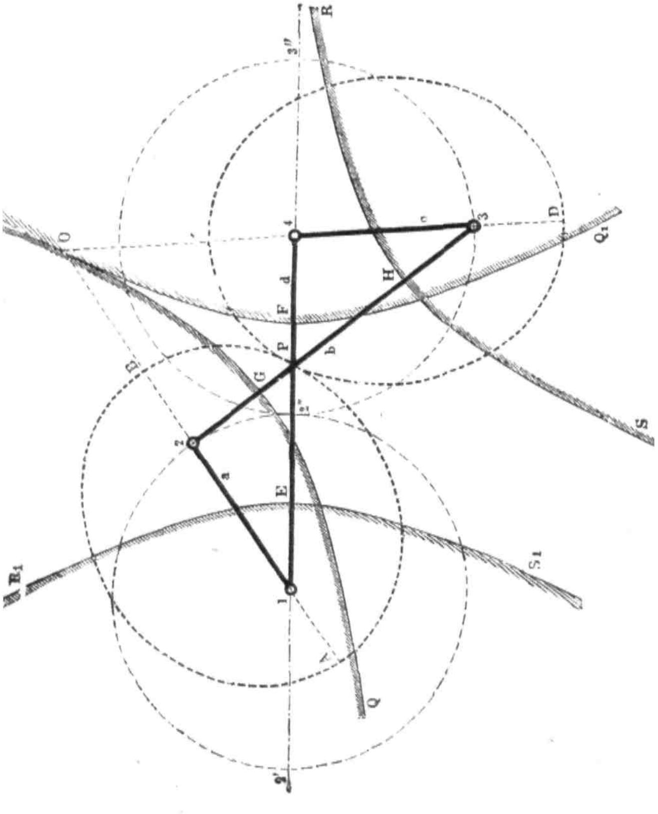
FIG. 8.

of Fig. 8) may serve as an illustration of this. When the crank *a* is at right angles to *d*, the normals to the paths of the two points 2 and 3 are parallel. The instantaneous centre of *b* relatively to *d* is therefore at an infinite distance. Each centroid has, therefore, a pair of infinite branches.

We may look, in conclusion, at one other case which possesses some special interest on account of the form taken by the centroids. It is shown in Fig. 9. The chain contains four links and four parallel cylinder pairs. The alternate links are equal, and the two longer links are crossed so that the chain forms an "anti-parallelogram" in every position, the angle at 2 being always equal to that at 4, and the angle at 1 to that at 3. If the link *d* be fixed, the links *a* and *c* become two cranks which revolve in opposite directions with a varying velocity-ratio. The centroids of *b* and *d* are a pair of hyperbolæ having their foci at 2 3 and 1 4 respectively. The one rolls upon the other as *b* moves, the instantaneous centre in the position shown being at the point of contact *O*, which is the point of intersection of 1 2 and 3 4. The centroids of the two shorter links are the two ellipses which are shown in dotted lines. They are confocal with the hyperbolæ, and their point of contact is always at the intersection of 1 4 and 2 3. Their form shows at once that the rotation of the axes 1 and 4 is precisely the same as that which would be communicated by a pair of elliptic spur-wheels having the centroids for their pitch ellipses.

In this mechanism, as in some of the others illustrated, the centroids of two *adjacent* links, as *a* and *d*, or *b* and *c*, are simply

a pair of coincident points which roll upon each other. They form thus a limiting case of centroids, but every theorem



which applies to the more extended centroidal curves applies also to these points, as can easily be seen on examination.

THE KINEMATICS OF MACHINERY.

LECTURE II.

WE shall in the present Lecture examine in some detail a few of the results which can be obtained by treating mechanisms upon the plan which Reuleaux has proposed, and which is illustrated by his models; that is to say, by the analytical treatment of which we have already seen the general nature.

We have seen how kinematic chains are built up from pairs of elements and links. The pairing and the linkage renders the relative motions in the chain absolutely determinate, and the determinate relative motion exists equally whether or not any link of the chain be fixed relatively to the earth or to any portion of space that we choose to treat as stationary.

We have now to consider in more detail the effect of fixing one link of the chain. In practice, of course, one link is always fixed, or in other words, its motion relatively to the earth, to a locomotive or whatever it may be, is made zero. A chain with one link fixed is simply what we know as a mechanism.

In examining pairs of elements we saw that we could fix either element of the pair with lower pairs, the relative motions remaining unaltered; with the higher pairs the inversion gives us a totally different motion. We have seen also that we can fix any one link of a kinematic chain just as we can fix either element of a pair. We therefore can get as many mechanisms from any chain as it has links. From any such chain as Fig. 3, for instance, which has four links, we can get four mechanisms. The fact that a kinematic chain gives us as many mechanisms as it has links appears, looked at from this point of view, a mere matter of course. It has, however, never been hitherto distinctly recognized, so far as I know, and it can hardly be realized too distinctly, the consequences which result from it being most important, as we shall see. All that I shall attempt to do in this lecture will be to look at some of the mechanisms obtained from the particular chain just mentioned, and various modifications of it.

We have already noticed that the chain has four links. We see further that it is a chain in which all the motions are con-

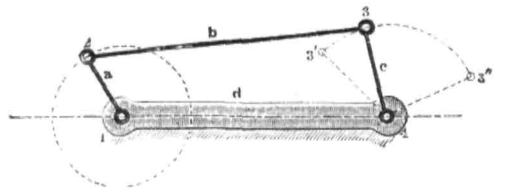


FIG. 10.

plane, each of its four pairs being simply a cylinder pair, and the four cylinder pairs having parallel axes. It is so proportioned that by causing one link to swing, another one can be made to revolve. In order that we may refer more easily to the links a letter is attached to each in the engraving.

For convenience sake we may also use a short symbol for this chain (the one used by Reuleaux) namely, (C_4'').[1] The C_4 within brackets stands for the four cylinder pairs, the symbol for parallel being added to indicate their relative positions. This is the symbol for the *chain*, no link being fixed. To distinguish the four mechanisms formed from it, we shall put the letter which stands for the fixed link in the position of an index after the formula. Thus we can denote the particular mechanism shown in Fig. 10, in which the link *d* is fixed, by the formula $(C_4'')^d$. We have here then the first of the four mechanisms we can get from this chain. You will recognize it easily enough as exactly similar to the beam and crank of a beam-engine. The link *c* is half the beam, *a* the crank and *b* the connecting-rod. The whole mechanism is an excellent illustration of what I said in my last lecture, that the *form* of the links is indifferent. If you think of the mechanism as forming part of a beam-engine, for instance, you will see in the link *d* the abstract form of what is generally a most complex structure, a bed-plate with its bearings, an entablature and plummer-block, cast-iron columns, and in some cases even brick and masonry. All these are represented by the fixed link *d* so far as their kinematic relations are concerned.

If now we fix the connecting-rod *b* instead of fixing the

[1] In words "C parallel 4."

link d as before, we have the mechanism $(C''_4)^b$. It does not essentially differ from $(C''_4)^d$. The crank now revolves about the pin 2 which was formerly the crank-pin, and the pin 1, which formerly represented the crank-shaft, is now the crank-pin, but there is nothing changed in the nature of the mechanism. By this inversion therefore we have got nothing new.

Let us now fix the link a, which was formerly the crank (Fig. 11). We have now the mechanism $(C''_4)^a$; it contains

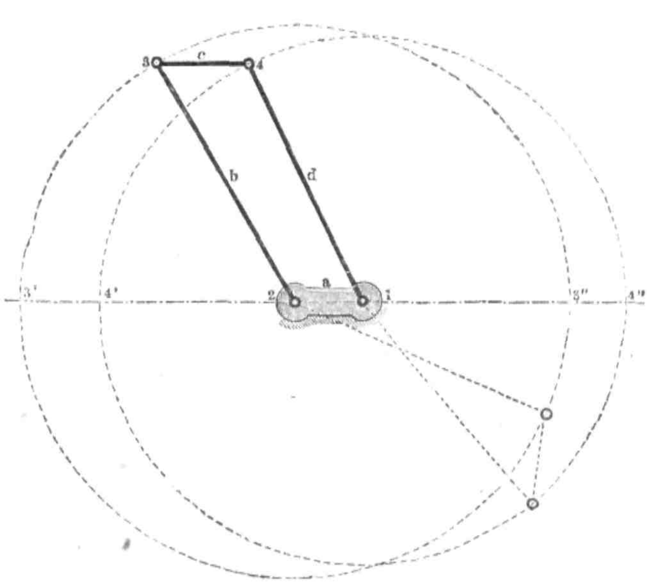


FIG. 11.

precisely the same elements as before, and the relative motions of the links are unaltered, but as a mechanism it is entirely different. It is now a *combination frequently enough* used in mills and elsewhere, known by the name of a "drag-link coupling." The links b and d have become cranks, and one drives the other by means of the link c.

By fixing the remaining link of the chain, the link c,

(which we have supposed to be longer than a), we have the entirely different mechanism $(C_4'')^e$, (Fig. 12). The two arms no longer revolve but only swing, and the link a turns right

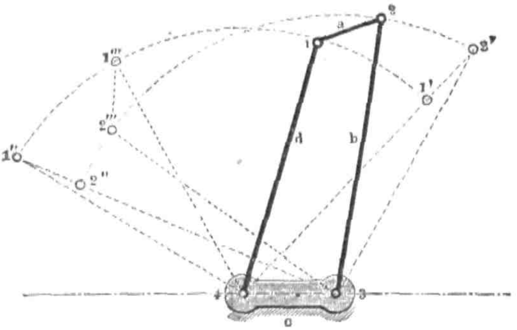


Fig. 12.

round once in every double swing of d and b. This mechanism is occasionally used in part of its stroke in parallel motions with some modifications, but is not so well known as the others.

The four inversions of this one chain therefore give us three different mechanisms. Looked at separately it is hard to see the relation in which these stand to each other; from the point of view which we have taken their mutual relationship has become at once evident.

Without altering the pairing at all, we can greatly alter the chain by changing the relative length of its links. If we made all the links equal we should have a square, of which all four inversions would give similar mechanisms. If we make $b=d$ and $c=a$ we get a mechanism which is perfectly familiar in the couplings of locomotives and many other cases. All four mechanisms are again similar, each one consisting of a pair of cranks revolving with equal velocities and connected by a link which moves always parallel to itself.

These mechanisms are among those which have the peculiarity to which I alluded yesterday, that in one of their positions their motions are not determinate. This occurs at

the "dead points" when a and c are both standing in the direction of the axis of d or b. If no means be taken to prevent it, it is then possible to move the crank either in one direction or the other, and the two cranks may go on revolving in the same direction, or may revolve in opposite directions according to circumstances.

Such an indeterminateness is, of course, inadmissible in machinery, where we therefore adopt the well-known method of combining two mechanisms of the same kind, and placing them with their cranks at right angles, so that they do not cross the dead-points at the same time. The motions are thus made determinate and the cranks revolve in similar directions. We might, however, wish them to revolve in *opposite* directions, as in the mechanism shown in Fig. 9. It may be worth our while to look for a moment at the means which may be used in this case to secure the determinateness of the motions in the mechanism. To distinguish between the two cases we may call the former "parallel cranks" and represent it by the formula $(C''_2 \parallel C''_2)$, and the latter "anti-parallel cranks," $(C''_2 \angle C''_2)$. This chain, with the link d fixed, is shown in Fig. 13.

In the case of the parallel cranks all points of the centroids of b and d are at infinity, for they are at the intersections of the parallel links a and c. We have already seen, however, that in the mechanism Fig. 9 the centroids are quite different, those of b and d being hyperbolæ. If, therefore, when the mechanism is brought into either dead point, where the cranks might change from the anti-parallel to the parallel position, we can only make certain that the right centroids roll upon each other, we shall get the

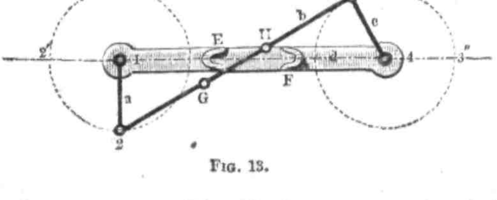


FIG. 13.

motion that we want. Fig. 13 shows an arrangement by which, just in that position of possible change, a tooth made

on one link and a recess upon the other link gear together at a point corresponding to the point of contact of the centroids. The teeth G and H are virtually formed upon the centroid of b, and the recesses E and F upon that of d. At the points where these come into gear, the two centroids are compelled to roll upon one another, just as the pitch circles of two toothed wheels are compelled to roll on one another, and in this way the mechanism is carried over its only indeterminate point, and the cranks remain continuously antiparallel and revolve in opposite directions.

This antiparallel chain gives us two different mechanisms. Fig. 13 *shows us* $(C''_2 \angle C''_2)^d$. *In the other mechanism* $(C''_2 \angle C''_2)^a$ the two cranks revolve in the *same* direction with very varying velocity ratios.

Returning again to the chain (C''_4), it will be seen at once that we may substitute for the pair of elements at 4 a slot and a sector concentric with it, as in Fig. 14. The motions

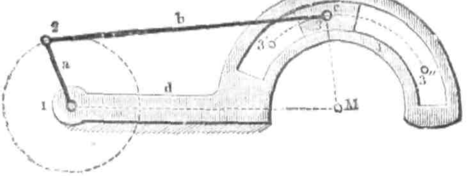


FIG. 14.

remain entirely unaltered. By adopting this construction, however, it becomes possible to construct the mechanism without covering with it the centre of the pair 4, *i.e.*, the point of intersection of the links c and d. We can therefore lengthen these links without making the mechanism inconveniently large. The only constructive alteration is that the slot becomes flatter as the links are lengthened. If we lengthen them little by little until they become infinitely long, the curved slot becomes straight, and its centre line will pass through the point 1. The mechanism modified in this fashion takes the extremely familiar form already shown in Fig. 8. It now contains three cylinder pairs with parallel axes; the fourth cylinder pair has become a straight slot with a block working in it, namely, a prism pair (see page

80). The axis of the prism pair is normal to the axis of the three cylinder pairs, and we may therefore use the symbol $(C''_3 P\perp)$ for the chain in its new form. There are here again four links, and therefore four inversions, and we shall find that all four mechanisms are now different.

We have first the mechanism shown in Fig. 8, and familiar by its continual use in direct-acting engines $(C''_3 P\perp)^d$. Next, following the same order as before, we may fix the link *b*, the connecting rod of Fig. 8. The mechanism thus obtained, $(C'' P\perp)^b$, is quite different from the former, but equally

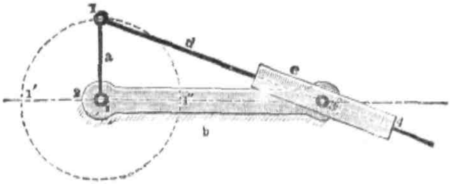


FIG. 15.

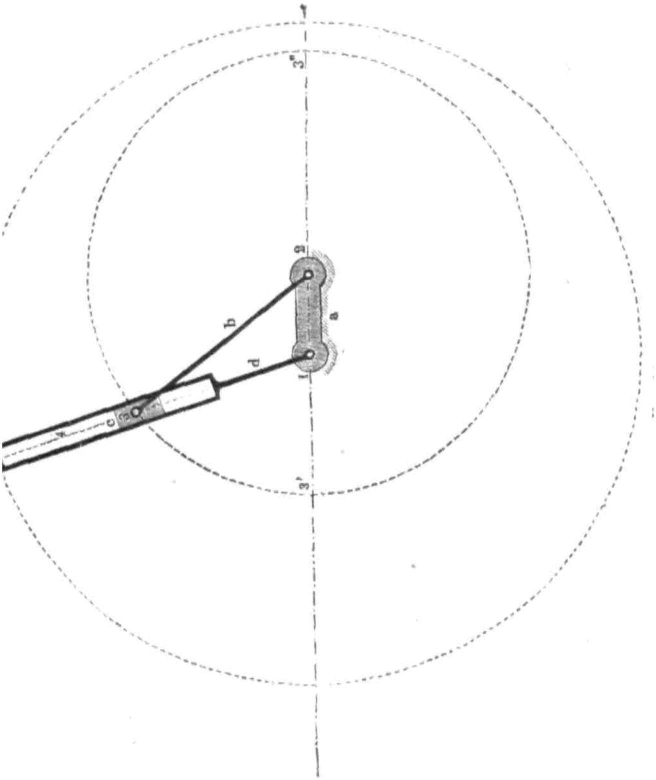
familiar (Fig. 15). To make it more recognisable, the prism pair 4 is *reversed* in the figure, that is the link *c* is made to carry the open prism and *d* the full one. The motion is obviously unaffected by the change. The mechanism can be easily seen to be that of the oscillating engine. The link *c* corresponds to the cylinder, swinging on fixed trunnions at 3, and the link *d* to the piston-rod and piston of the steam-engine. We see then that the relation between the mechanisms which are familiar to us as the driving-trains of the direct-acting and oscillating engines, is simply that they are different inversions of one and the same chain.

Let us now suppose the chain fixed upon the link which was the crank in the last two mechanisms (Fig. 16). This gives us a third mechanism which entirely differs from either of the two former ones. It is quite familiar as a "quick-return" motion in some machine tools, for which purpose also the mechanism last mentioned has sometimes been used.

Fixing, lastly, the link *c*, we get the less familiar mechanism shown in Fig. 17 $(C''_3 P\perp)^c$. The link *b* swings about 3, and the crank *a* rotates in space somewhat as in the mechanism

$(C_4'')^e$, which we have already seen. This train has some practical applications in machinery, but is not very often used.

Here then we have obtained from one and the same chain



four entirely different mechanisms, all of them more or less familiar. The method we have adopted has again been successful in making the real relations of these apparently dissimilar things perfectly obvious.

We have seen in connection with Fig. 14 that we can to a certain extent alter the size and extent of a pair of elements without altering its nature or changing the motion of the chain to which it belongs. This alteration in the size of elements, or what may be called the "expansion" of elements, is a process continually carried out by engineers for practical constructive reasons; and often gives to identical mechanisms extremely different forms. It is impossible here to go into this in detail, a somewhat extreme case of it is shown, for the sake of illustration, in Fig. 18. Here we have the mechanism shown already in Fig. 8, $(C''_3 P\perp)^d$. The pin of the pair 2 is so enlarged as to include altogether the pair 3, the connecting-rod *b* being simply a circular disc, with an eccentric cylindric hole in it. The pin 1 again (the "crank-shaft"), is made large enough to include the whole of 2. We have therefore 3 within 2, and 2 within 1. We have one very common illustration of the extent to which this expansion of pairs is carried practically

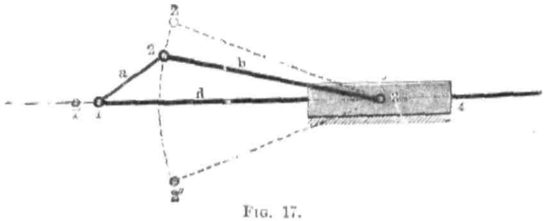


Fig. 17.

in the link motion. The curved link and block are in their kinematic relations simply a very much expanded pair of cylindric elements reduced in extent by use of a process similar to that by which we got Fig. 14 from Fig. 10.

We have seen what results have been obtained by making two links of the chain (C''_4) infinitely long. The same process can be carried still further. In the familiar chain shown in Fig. 7, for instance, *three* links, *d*, *c* and *b*, are made infinite. We have therefore another prism pair in it, and its formula becomes $(C''_2 P\perp_2)$. It gives us the two mechanisms already mentioned and a third one, all of which are practically applied.

We must pass over without mention many other modifications and alterations of the chain, and mention only one other form in which it occurs, a form which has some special interest. The condition of movability of a chain that contains four cylinder pairs is not that their axes should be parallel, but that they should *meet in one point.* The axes are parallel only in the special case where this point is at an infinite distance. Fig. 19 is an illustration of the more general, although less familiar, case when the point of intersection is at a finite distance. This chain, which may be

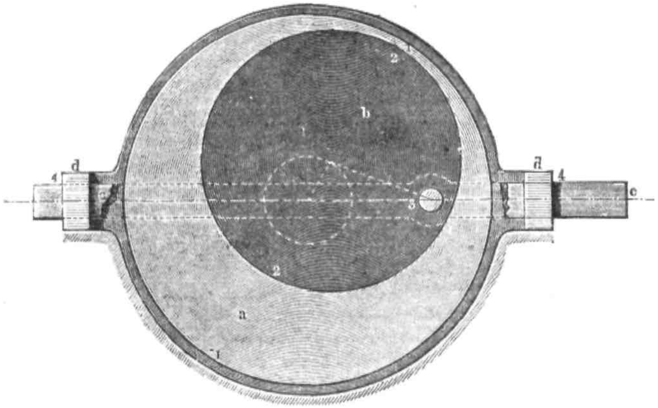


FIG. 18.

indicated by the formula $(C^{\angle}_4)$ [1] has again its four inversions, and furnishes us with three different mechanisms as in the case of (C''_4). I cannot here go into these; I mention the chain partly because of its theoretic interest, and partly because although it looks so unfamiliar, it is not unfrequently applied in machinery. If instead of subtending only a small angle, as in Fig. 19, three of the links were made to subtend an angle of 90°, we should have the common Hooke's or universal joint. In these "conic" chains, links which are quadrants take the place of the infinite links, so that a chain having three quadrants corresponds to the chain of Fig. 7,

[1] "C four oblique."

in which there are three infinite links. The formula of the universal joint is $(C^{\perp}_{8}\ C^{\llcorner})^{a}$; it corresponds exactly to $(C''_2 P^{\perp}_{2})^{a}$ and is analogous to $(C''_4)^{a}$ and $(C''_3 P^{\perp})^{a}$, Figs. 11 and 16 respectively. I have already mentioned in passing that the mechanism $(C''_2 P_2{\perp})^{a}$ is that of Oldham's coupling. We see then clearly the close relationship in which this *coupling (for parallel shafts) stands to the Hooke's joint* or coupling for inclined shafts. It is another illustration of the important results which follow naturally from Reuleaux's simple method of analysis, and which hardly seem attainable, certainly not with equal directness, by any other method.

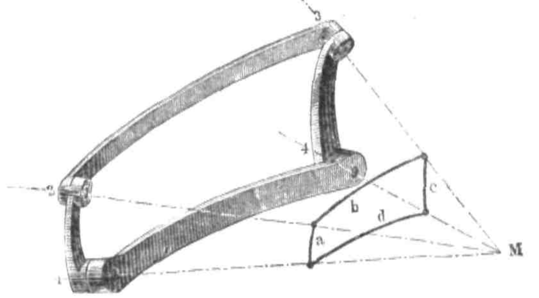


Fig. 19.

We may notice one other way in which the chain (C''_4), which we have seen in so many forms, may be modified. In some cases we may not wish to utilize the motion of all the four links, but (say) only three of them; in that case we may omit the fourth link, if we carry out a proper pairing between the two links thus left unconnected. If, for instance, we omit the link *b* we must make a proper pairing between *a* and *d*. This pairing will always be higher; the chain becomes a *reduced* chain. Such a chain, reduced by *b*, is shown in Fig. 20. The higher pairing is carried out by placing upon *a* a suitable element, here a circular pin at 2, and giving to *c* the form of the *envelope* (see p. 5) of the motion of the pin relatively to it. This envelope is the curved slot shown in the figure. It was not necessary to take the new element at 2, but if it had been in any other place, as 2^1, the form of the slot would have been more

complex, as is shown by dotted lines. For such a chain we may use the formula $(C''_3\ P\perp) - b$. The process of reduction can be carried on in this way until only two links are left, which then become really a pair of higher elements. It is constantly employed in machinery, mostly in the case of compound chains, or chains in which some links contain more than two elements.

The chain which we have been examining has been applied more often than any other to the leading trains of engines and pumps. We shall in conclusion look at a few of these machines, in order to notice the constructive disguises which often appear in them and render their kinematic identity almost unrecognisable.

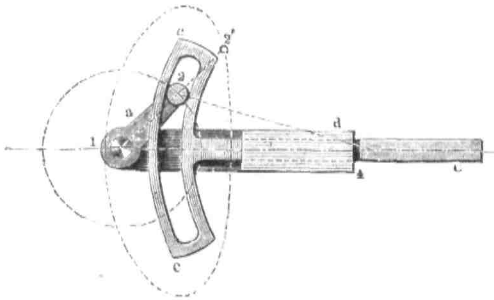


FIG. 20.

Fig. 21 shows a rotary engine which has been patented several times, and which is founded on the same mechanism, $(C'''_3\ P\perp)^d$, as the common direct-acting engine. The letters and figures placed upon it correspond to those on Fig. 8, so that the identity of the mechanisms may be the more easily traced. The extraordinary change of form undergone by the connecting-rod *b* is worth special notice. It has become a bar having a cross-section like a half-moon. It still consists however, kinematically, of two cylinders or portions of them, one described about the axis of 2 (the centre of the disc), the other about the axis of 3. The element 2 is expanded so as to include 1, the crank *a* becoming therefore a disc. The mechanism is so proportioned that the virtual length of the connecting-rod—the distance, that is, between the centres

of the elements 2 and 3, is equal to the radius of the disc *a*. With the mechanism in the form shown in Fig. 21 some separate means has to be provided for keeping *b* against *a* when the latter is moving upwards.

Fig. 22 is another rotary engine, which has been patented a dozen times since its first invention in 1805. It is based upon the mechanism of Fig. 16 $(C''_3\ P\perp)^a$. The fixed link *a* is here made the steam cylinder, while *d* becomes a

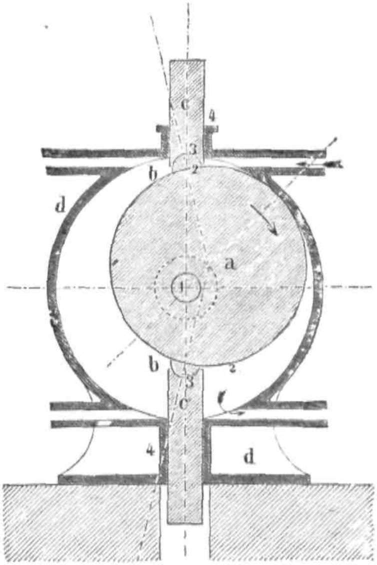


FIG. 21.

moving piston. The reference letters are the same as before. It will be noticed that the cylindric element of the link *c* is expanded to include its prismatic element; in all the cases formerly noticed the latter had been the larger.

In order to illustrate this part of his subject Reuleaux examines (in the work I have already mentioned) some forty or fifty rotary engines and pumps all derived form the (C''_4) chain and such modifications of it as we have been looking

at. Models of a number of these are now on the table before you. Many of them bear scarcely any external resemblance to the common steam-engine, to which they are, notwithstanding their dissimilarity, so closely related. We are not now concerned as to which form is absolutely the best, but are only looking at them from a kinematic point of view. But it is worth noticing that in very many cases not only constructive but mechanical advantages have been claimed for them. Their inventors have over and over again

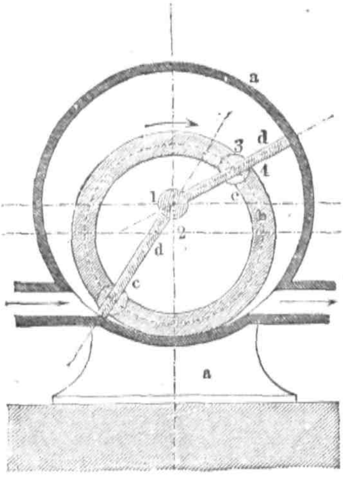


FIG. 22.

claimed some mechanical gain more or less mysterious, compared with the ordinary form of engine. Most of them, moreover, have been called "rotary" to distinguish them from reciprocating engines. Our method of analysis, although only kinematic, has shown us not only that there can be *no* mechanical advantage possessed by one over the other, but that the word "rotary" is essentially a misnomer, if it be supposed to indicate that there is any more or different rotation in them than in ordinary engines.

We shall now only notice one more of these engines. It is one which has puzzled people a great deal, and with very

good reason, for its motions are very strange, and its analysis apparently very complex.

The engine I refer to is shown (in one form) in Fig. 23. It is known as the "disc engine." It was brought a good deal into notice in 1851, and for a few years was used in the *Times* office without any ultimate success. Analysis shows that it is based upon the same chain as the Hooke's joint, the conic chain, namely, in which three out of four links subtend right angles, and of which the formula is $(C^{\perp}{}_3 C^{\angle})^{s}$. The fixed link is, however, *d*, while in Hooke's joint

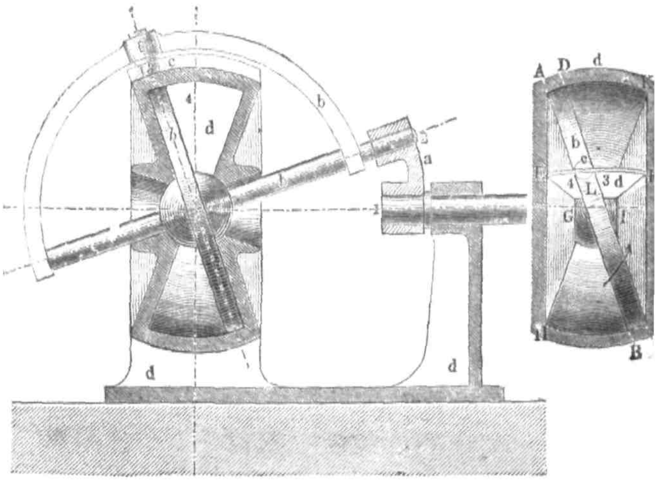


FIG. 23.

a (the acute-angled link) is fixed. Patents have been taken out also for disc-engines in which *a* and others in which *b* is the fixed link.

I may say, in conclusion, that while it may be impossible for many educational institutions in this country to possess themselves of models so perfect in execution, and therefore so expensive, as those which have been sent to the Loan Collection, it is yet quite practicable to construct many of them in a form which, while quite cheap, is yet well adapted for

educational purposes. I have some made in this way of hard wood with brass pins, which are very serviceable for college purposes. It is just the simpler models, which are the most easily made, which are the most useful in instruction.

I venture to hope that the treatment of the theory of mechanism illustrated by the models now in the South Kensington Collection, and of which I have here endeavoured to set forth some leading principles, may prove valuable in aiding the study and the comprehension of this branch of machine science both to the student and the engineer.

ON THE STEAM-ENGINE.

TWO LECTURES.

BY F. J. BRAMWELL, ESQ., M. INST. C.E., F.R.S.

LECTURE I.

Among all the subjects to which science has been applied, it must be universally admitted there is none more thoroughly and generally important than that of the steam-engine. By the aid of the steam-engine we traverse the sea, by its aid we go at the rate of fifty miles an hour upon railways, by its aid we manufacture every article we wear, we prepare the food that we eat, and we even prepare the ground to receive the grain which we convert into food; by it we get rid of our sewage, we supply ourselves with water, and, although the creature of fire, by it we extinguish fires; in fact, it is not too much to say that either directly or indirectly, there is *no one* thing which enters into the ordinary life of a civilised being to the preparation of which the steam-engine has not contributed.

The subject is so vast that I feel the greatest possible difficulty in dealing with it in the course of two lectures; in fact, so great a difficulty that I hardly know how to begin, or what points to touch. As to the points which must be left untouched, they will of necessity be very many indeed. I think you will be of opinion that (whatever we may be compelled to exclude) it would be impossible, in the neighbourhood of this science collection, to refrain from some allu-

sion to the history of the steam-engine, and you will see by the models on the table that that is the conclusion to which I have already come. Therefore I trust you will bear with me while I occupy some portion of your time in going into the historical part of our subject.

I begin with Papin, who, according to his pamphlet, published in 1707 (in consequence, as Belidor says, of a letter written from London in 1705), states that in 1698, he, Papin, made experiments with his steam-engine, the engine which, no doubt, is so well known to most of you. In the collection there is a cylinder (No. 2007) which is said to be a portion of one of Papin's machines. How that can be, I am at some loss to understand, inasmuch as the cylinder is open at both ends, and at one end only is there a flange or any provision for making a reclosure. The other end, it seems to me, must have always remained entirely open, and therefore I do not know how it could have been employed in any way in Papin's apparatus, of which there is an elementary diagram upon the wall. From this diagram you will see that Papin had invented that which may be called a high-pressure water-raising engine; but mere water-raising was not the limit (in intention, at least) of Papin's scheme, inasmuch as he proposed an alternative use of the pressure in the air-vessel, viz.:—the production of a powerful jet of water from the outlet cock, which jet should impinge on the vanes of a water-wheel, and thereby produce rotary motion. Whether rotary motion was ever so obtained does not appear to be recorded. Although, as I have said, no doubt Papin's engine is well known to most of you, it will be well briefly to describe it. It consisted of a boiler, from which a steam-pipe proceeded to the upper part of a vertical cylindrical pressure-vessel, which could be filled with water from any source of supply above it; then, the inlet of the water being stopped by the turning of a cock, the steam from the boiler could, by the turning of another cock, be admitted to press upon the water in the vessel and to drive it outwards through a check-valve into an air-vessel, whence it was to rise to the desired height. To diminish the condensation of steam, a floating diaphragm (which some call—and, to my mind, improperly call—a piston) was provided to prevent the actual contact of the steam and the water.

At about the same time Savery, (as we know) in England, was not merely making experiments but was bringing into

actual use his water-raising engine. We have a model of that engine here. It comprised the steam-boiler connected by a pipe (controlled by a cock) with the water-vessel. Steam being admitted and then shut off, a water-cock was opened, which allowed a cold jet of water to play over the outside of the water-vessel, condensing the steam and making a partial vacuum within the vessel, and thereby enabling it to "suck up" (to use a most convenient but unscientific term) the water from the source of supply; which water, being retained by a suction-valve, was on the next admission of steam forced through the delivery-valve to the desired elevation, the difference between Savery and Papin being that Savery made use of exhaustion to do part of the raising, and that he did not have a floating diaphragm between the steam and the water to diminish the amount of steam lost by condensation. The engine of Savery was put to work and used to a considerable extent, notably at the waterworks, York Buildings, in London, which were situated where Charing Cross Station now is.

As I have said, Savery's engine did not remain in the region of idea, nor even in that of mere experiment, but it was put to work in several places, and was thus fully tested. These testings, while they showed the merits, showed also the defects of Savery's invention, and these defects were of special importance in days when leaden pipes and plumbers' joints had to be relied on to resist high pressure of steam; for you will see that in a water-raising engine such as this (as also in the engine of Papin) the steam acting directly upon the surface of the water to be raised must press with a force per square inch equal to the balancing and to the setting into motion (against the resistance of friction as well as that of gravity) of the water to be lifted, and that thus to raise water, say 115 feet, would demand (friction apart) a steam-pressure of 50lbs. above atmosphere, or probably, allowing for the friction and the loss by condensation, 60lbs. or more in the steam generator.

With an apparatus such as this, therefore, no alteration either of dimensions or proportions could enable you to evade the fact, that the steam pressure per square inch must be sufficient, not only to balance the column of water, but also sufficient to overcome the friction and all other resistances. Under these circumstances, it is by no means surprising that

the mechanical skill and appliances of those days were unable to cope with the demands made upon them, and that pipes, joints, and cocks, leaked and gave way.

It was at this time, while the new steam power was struggling into existence, that Newcomen (the father of the modern steam-engine, as I believe him to be) came forward with that invention which in its leading features is used by us to this day. Newcomen said :—I will show you a mode of making steam raise water where the pressure of steam per square inch need not bear any relation to the height to which the water has to be lifted ; I will show you that the relation between the resistance and the power necessary to overcome that resistance may be adjusted by causing the steam to act upon some surface not that of the water, and that this surface may be a solid moving freely (but practically steam tight) in a cylinder (this surface being the true piston at last), and that by suitable means I can cause this surface acted upon by the steam to work the other surface whicg is to act upon the water, and that by the due adjustment of the relation between these two surfaces I may make steam of low pressure (or as in practice I intend to effect it, a portion of the pressure of the atmosphere) master of a resistance of hundreds of feet of head of water, and that thus steam barely powerful enough to overcome the atmospheric resistance shall master the resistance involved in the raising of water from out of the deepest mine, or shall produce the high pressure required to distribute water throughout the contracted and tortuous wooden mains for a town supply.

An engine of this description (a Newcomen engine) was put up in York Buildings to come to the rescue of, and to supersede, the Savery engine which I have previously described.

Belidor (that most conscientious of writers) states in his *Architecture Hydraulique*, that he heard of this engine succeeding Savery's at York Buildings, and that he then visited one which was put up at Fresnes ; this he did several times to enable him, as he says, to do justice to it, and to give his readers full information upon it. He has given the fullest possible information, and has left us drawings so complete that any workman in an engineer's factory could at the present day reproduce from those drawings the very engine that Belidor wrote of in 1737. The leading dimensions were as

follows :—Cylinder thirty inches diameter, stroke in the cylinder and in the pump six feet, fifteen strokes per minute, and depth of pit 267 feet. The pumps were seven inches in diameter, twenty-four feet apart, and pumped from cistern to cistern, from the bottom to the top. The boiler was nine feet diameter, three feet deep in the body part, and on the top there was a curious precaution, namely, two feet six inches of masonry to keep that top from being lifted by the pressure of the steam. There were to this engine, gauge-cocks, a safety-valve, and the injection into the cylinder of those days—Newcomen's injection, and a piston kept tight with leather packing with water on the top. The result of the erection of this machine was, that whereas previously fifty horses and twenty men had been employed day and night continuously to keep the colliery free of water, when the engine was completed forty-eight hours of its work unwatered the colliery for a whole week, it requiring only one man on guard to attend to it. Having described this machine in the most detailed manner, Belidor breaks out into a rhapsody upon it, which being roughly translated is to this effect: —"It must be acknowledged that here is the most marvellous of machines, and that there is nothing in mechanism which relates so nearly to animal life. Heat is the principle of its movements, there is passing in its different tubes a circulation like that of the blood in the veins; it has valves which open and shut at the proper times, and it performs all the functions which are necessary to keep it in existence, and to derive from its work all that possibly can be desired." I think those enthusiastic remarks of the excellent writer Belidor, in 1737, are as applicable at the present time as they were then, for although the machine is no longer a matter of novelty, and we have got used to it and to its benefits, as we have to those of light and air; and have thus lost the very appreciation of the existence of its benefits, as we have those derived from light and air, until from some cause they are forced upon our notice. We must, nevertheless; when they are forced upon our notice, agree with Belidor that there is not any machine the product of the intelligence of man which is more worthy of our consideration and commendation than is the steam-engine.

Of the Newcomen engine I have a model here, lent by the Council of King's College. This is such an engine as

Belidor describes—the boiler with the cylinder above it, the beam, the pumps worked at the other end of the beam, the open-topped cylinder with a piston kept tight by water packing, the water injection into the cylinder, and the plug-tree to make the valve self-acting, and to cause the machine to continue at work. I have also here, lent by Mr. Bennet Woodcroft, one of the most interesting engravings to an engineer that I know of. It was published in the year 1719, and shows the Newcomen engine erected in 1712 at Dudley Castle. I have also before me another model of a Newcomen engine which is of historical interest. This is a model lent by the University of Glasgow, and bears the label that "in 1765 James Watt, in working to repair this model belonging to the Natural Philosophy Class in the University of Glasgow, made the discovery of a separate condenser which has identified his name with the steam-engine."

To Watt we owe, as you know, the condensation in a separate vessel, the parallel motion, steam-jacket, double-action, expansion, the indicator to tell us how the engine is working, and the governor. We have on the table before us, models brought from the collection, and bearing upon these various subjects.

Passing on from Watt, we get to Hornblower, who in 1781 patented the double-cylinder engine, afterwards largely developed by Wolfe. Leaving the general history and going to a particular class of steam-engines, and going back a little in date, I must refer to Jonathan Hulls, who in 1737 published the pamphlet I have here, in which he shows how, by the use of the Newcomen engine working a stern wheel, a paddle boat could tow ships out of a harbour against an adverse wind, or in the absence of any wind. As the engraving appended to Hulls' pamphlet is not large enough to be distinctly visible, I have had a diagram made of it which is upon the wall. Hulls states that he proposed to employ a Newcomen engine, the rectilinear reciprocating motion of which being obtained by the alternate action of the pressure of the air on the piston for one direction, and of the descent of a counterweight equivalent to half the effect of that pressure for the other direction. Hulls employed this to give alternating reciprocating motion to two pulleys placed upon a shaft, from which by means of bands, one being open and the other crossed, motion was to be transmitted to the

paddle-shaft. At each reversal of this shaft, the band which had been driving became slack and ceased to drive, while that which had been slack became tight and did the driving. Obviously, the alternate coming into operation of an open and of a crossed band worked from a shaft driven first one way and then the other, would communicate to the paddle-wheel the desired motion in the one direction.

Further, on the subject of steam navigation, we have in the collection the actual machine (No. 2150) which, in 1788, worked on Dalswinton Lake in Scotland and drove a small boat. We have likewise the actual engine of Bell which drove the first passenger steamer in Europe.

Leaving navigation, and going to another branch of the subject, namely, steam as applied to land transit, I must refer you to the extremely interesting model (No. 2145) of Cugnot's steam carriage, which, in the year 1769 traversed the streets of Paris carrying passengers. It went but very slowly, only some two or three miles an hour, and it had frequently to stop to get up steam, but nevertheless it was an actual working common road locomotive, and is therefore a model of great interest, looking at the date at which the original engine was put to work. You will see it is a three wheel carriage, the engine and boiler being attached to the front wheel, so that when the front wheel was slewed by the steersman for the purpose of altering the direction, the whole engine and boiler canted with it. Very curiously (it may be in the recollection of some of you) two or three years since, Mr. Perkins exhibited in the grounds attached to these buildings a common road locomotive, which like Cugnot's was a three wheel machine, and like it, had the engine applied to the castor wheel, and turned boiler and all, with the castor wheel as the steersman moved it.

In Cugnot's engine we have another instance where rotary motion was wanted, and where there was no provision for obtaining it such as we have now-a-days by means of a crank. Cugnot overcame the difficulty by employing two direct acting inverted cylinders which, by means of palls operating on ratchets, kept the driving wheel revolving always in the desired direction.

Passing from Cugnot, I now come to Trevithick's engine, a model of which we have here. I was about to ask you to look at the Blenkinsop rail, a sample of which, belonging to

the Institution of Mechanical Engineers, is shown in the Exhibition. It is a rail which, as you know, was provided with teeth projecting from one side of it, into which cogs made in a deep flanche of the bearing-wheel took, so as to ensure that that wheel should move the train and engine and should not slip upon the rail. In Blenkinsop's time this was thought to be a necessity, and such rails were laid down to a considerable extent. I find the sample rail has not been brought in, and I therefore content myself by saying, that a piece of such rail, which was for many years in actual use, is in the collection. In the building also we have the original Puffing Billy, the first engine which traversed, I believe, for practical purposes, a smooth rail, and an engine which worked from 1813 to 1862, and then was placed in its honourable retirement to be revered by engineers. Alongside of that we have Stephenson's "Rocket." That engine is generally thought to be in the original condition in which it worked, but a very cursory examination will show that is not so, and those who have read last Saturday's *Engineer* will have seen a criticism on that Exhibit, pointing out that there have been material alterations, and that therefore it should not be looked upon as showing the exact state in which the engine was put to work in 1830. The other day when calling on Mr. George Robert Stephenson, President of the Institution of Civil Engineers, and now the representative of the Stephenson family, I looked at a model in his possession of the "Rocket," a model which had been made by a man who had driven the engine for many years, and when I found there was doubt cast upon the extent of the alterations, which the real engine had suffered, I thought it would be well to get the model here, and here it is on the table. There is no doubt whatever that this model corroborates the statement in the *Engineer*, and shows you really the engine as it worked in 1830, without the alterations which have been made in the actual engine now deposited. The alterations, although rather numerous, were not important. In the original engine, as shown in the model, the cylinders were at a considerable angle. You will find that in the real engine they lie at a much less angle. In this model also there is no smoke box, and in the engine there is. I do not know that there are any really important points of difference, but nevertheless when a thing is exhibited for its historical interest, it is as well you

should see an exact representation of that which existed at the date under consideration, and I am much obliged to Mr. George Robert Stephenson for having kindly lent us the model for this afternoon's purpose.

I have already said, I think, we must look upon it that Newcomen was the first man to make a steam-engine, as we now understand it, an engine with a cylinder and a working piston. Papin, it is true, had a piston, but it was a mere diaphragm acting as a non-conductor between the steam and the surface of the water, and it cannot therefore be regarded as the piston of a working engine. I think we can very readily see why it was that we came to have the Newcomen engine in the form of the beam engine originally. The piston was kept tight by water-packing on the surface of it; if any of the packing leaked through, it did a little harm of course by cooling the steam coming to the cylinder, but not very much, because the piston rose rather by the action of the counterweight than by the pressure of the steam, and any water which leaked through aided the injection in the return (the working) stroke. But to use water-packing it was necessary that the cylinder should be vertical, and that the piston should be pressed downwards in order that the water-packing might lie on the piston. If you press the piston downwards, as in those days was done, and did not work with plunger pumps, but with lift pumps, it involved the use of a beam, which, when the piston went down, pulled the pump upwards, and in this way originated the beam type of steam-engine. Now it is extremely difficult in engineering, as in many other matters, when we have once got into the habit of doing a thing, to get rid of that habit. One is apt to take that which is a mere adjunct as a necessity. If you will pardon me, I will tell you, as germain to the subject, that which Mr. Robert Lowe told the Political Economy Club the other night at the Centenary Festival. He quoted the novel of "Hadji Baba," by Morier, where a Persian, visiting England and admiring a young lady, is desirous of making her an offer, and being hospitably received at the house of an English friend, he asked him: "How did you propose to Mrs. Brown?" "Well," says the friend, "the fact is, one day we were going to church together and I offered her my umbrella, and under the shade of that umbrella I proposed to Mrs. Brown." Now the

Persian made the mistake of supposing that the umbrella was a necessary adjunct to a proposition for matrimony in England, and waiting until he could get a propitiously wet day for the purpose, lost his chance and the young lady. I should have been ashamed to take up your time with telling a tale such as this, had not a grave man like Mr. Lowe thought it not incompatible with his dignity to offer this story to the Political Economy Club as illustrating the mistakes those foreign nations make, who, when wishing to settle a question of tariffs, look upon a treaty of commerce as the vital matter, and not as a mere adjunct, the fact being that the treaty has no more to do with it than the umbrella has in the proposal of marriage. As a fact, however, when your mind is once possessed by an umbrella, or a treaty, or any other thing, it is extremely difficult to get free from it, and therefore when you had once, for any reason, settled on a beam engine it was extremely difficult to get rid of the beam and go to work without it. Besides that, there really were many conveniences attending the use of the beam; the different parts balanced each other, and beams afforded a very convenient means of attaching the pumps, the air-pump and the cold-water-pump and the feed-pump and of making their respective strokes proportionate to the duty required of them. It also, in olden days, was a convenient means of working the plug tree which operated the valves for admitting the steam and for exhausting it. The beam, therefore, had very many conveniences, but we see that when Watt closed the cylinder at the top and made the piston tight without water-packing, one of the very first things that occurred to him was that you might do without a beam at all, and might use the direct action of the piston, by placing the steam cylinder over the pit, and might by an invention of the piston rod, raise the pump without the aid of the beam—the construction now so well-known as the Bull engine. But still the beam remained for many years. It had, as I have said, its advantages, and so long as we were using steam for about 3lbs. pressure above atmosphere (that was the pressure when I was an apprentice), and with some 12lbs. vacuum; and so long as we employed house-engines, the beam being supported on girders built into the engine-house wall (either into the side walls, or in the case of a pumping engine into the end wall, the house only covering half of

the engine), no great difficulties arose. But looking on this beautiful model of a six-column engine (familiarly known as a "bed-post engine"), the favourite type of some thirty years ago for factory purposes, you will see that such a machine, when subjected to rapid alternations of strain, due to the use of high-pressure steam and of great expansion (on which important points I hope to address you in my next lecture), would be little competent to bear such strains; for a machine to support them must have its parts well tied together, and our American friends, in the beam engines which they have used with so much success in river steamers, have appreciated this fact, and therefore instead of a "bed-post" frame support of this kind, their walking beam, as they call it, is generally at the apex of a triangular structure of very great strength and of large base, but nevertheless even the American frames are very cumbrous, and one can see how desirable it is to arrive at a more simple form of engine. Notwithstanding this desirability, the beam remained, and when we came to paddle-engines we still used the beam, although we inverted it. I have here a model which is really of historical interest, lent by Messrs. Maudslay, of the engines of the "Great Western," the first paddle steamer built for the purpose of crossing the Atlantic, though I do not say it was the first which actually crossed it. Here is the model of the inverted beam engine of forty years ago, and I am sorry to say that the engineers of those days cared more that the framing should accurately represent a gothic window or an Egyptian temple, or some other piece of architecture, than that it should be designed to resist the strains which came upon it, and as a fact, they made an elaborately ornamental but weak frame which had to depend on the strength of enormous keelson, in the bottom of the ship to keep the parts of the engine themselves in position. Notwithstanding the strength of the keelsons, one frequently found the frames broken and patched with wrought iron.

By slow degrees we have, however, come to the direct-acting engine—the Bull engine, which I have shown you, is an early example of it. Then we had some forty years ago or more the horizontal engine, made by Taylor and Martineau, that type of engine which is now probably more commonly used than any other. Then we had by the elder Brunel an engine with the two cylinders inclined at an angle of 45 degrees each to the horizon, and by their connecting rods laying hold of

one crank pin, the cylinders being placed on an A frame; the construction known for many years as the Thames Tunnel engine. Then we had by Maudslay (and used in the Richmond steamboat, I think, as long ago as 1827) the oscillating engine, a most compact form, and one which has been largely employed for marine purposes. Here we have a model of that oscillating engine applied "Thames Tunnel" fashion, that is, the two cylinders at an angle of about 45 degrees to the vertical, and their piston rods laying hold of a single crank. We have then in the way of direct engines, the direct overhead engine, where you have a vertical cylinder, and a connecting rod leading up to the crank; and those engines, or a pair of them, are much used now-a-days as winding engines in collieries, the drum or a pair of drums between the two cylinders making the fly-wheel. Then we have the same kind inverted with a cylinder carried on side frames, and the connecting rod working a crank shaft near the level of the ground, a construction known as the "steam hammer" and largely used in screw steamers. Further illustrative of this construction we have a wooden model of the compound cylinder engine by Messrs. Rennie of the Peninsular and Oriental steamer "Pera," a form of engine which admits of very ready bracing to withstand the strains which come upon the various parts.

The locomotive, of course, is a grand example of the direct-acting horizontal engine, and so are most of our agricultural engines. Besides this, we have had the trunk engine which has been very largely used, either in the single trunk or in the double trunk form (a trunk coming out at both ends of the cylinder), where the piston body and the trunks working through stuffing boxes are the guides to take the inclined thrust of the connecting rod. There are doubtless some other instances of direct-acting engines which do not occur to me at the moment.

Except for the purpose of pumping and for hammering iron, and for matters of that kind, we almost always want the power developed by a steam-engine to be delivered in the form of rotary motion, and one would naturally say, "Why do we make these machines, beginning with reciprocating and ending with rotary motion? why do we not obtain rotary motion to commence with?" I need not tell gentlemen here that this has been the problem of the mechanician from the time of Watt to the present day. I do not propose to enter

into hardly any of the schemes devised for this purpose, but as you know, for some reason or another, it has been found not desirable up to the present time at least (for I am sure I should be sorry to limit invention) to employ engines of that kind. At least so far as I know there is not one in practical work; although there have been some extremely ingenious suggestions. The Earl of Dundonald (Lord Cochrane) made a celebrated rotary engine at the place where I was apprenticed. It worked either with air or with steam, and so long as we worked it by exhausting air and letting the atmosphere press upon it, it ran satisfactorily, but when we put steam into it, and it had to support the variations caused by expansion, there were always difficulties. Nevertheless Lord Cochrane was a bold man, and went on. We made a locomotive for the Greenwich Railway, he would design his own boiler, and that boiler never raised steam to speak of; and the only result of the steam that was raised was, I am sorry to say, the destruction of the engine. Earl Dundonald, good sailor as he was, had forgotten his anchorage, he had only anchored his engine by the steam pipe, and when the steam was put on, the engine preferred to twist the steam pipe round instead of causing the driving wheels to revolve, and the unfortunate locomotive was consigned to the scrap heap. There is at the present time under trial a rotary engine of the most extraordinarily simple character. It is the invention of a Scotchman long resident in America, Mr. Dudgeon, who may be well known to many of you as the inventor of the "tube expander." This engine consists of nothing at all but two spur wheels with teeth that work steam tight in each other, and at their ends work steam tight also against two plates. Assuming the spur wheels to be on horizontal axes placed side by side, and that it is intended they should revolve so that the teeth which are in contact move upwards, then the steam would be introduced through one of the end plates a little above the line of centres; it would there be received into the cavity between two pairs of teeth, and it would press upon the under teeth to drive them down, while it would press upon the upper teeth to drive them up. It might be thought that under these circumstances no motion would ensue; but it will be seen that the area of the upper surfaces would be larger than the area of the lower, and thus there would be a preponderating upward effect. After the teeth have passed a particular

point they act as a slide valve and cut off the steam, and the steam contained between two teeth goes on expanding until a further point is reached where by their divergence they allow it to escape. I have seen the engine at work, and most certainly 60 lbs. of steam went into the engine and the exhaust steam came out perfectly inert. My notion was, that the 60 lbs. steam was being utilised. I have endeavoured to get results from trials by the dynamometer, but those trials were inconclusive. I merely bring this engine before you as one of the ingenious attempts—what its future may be I know not—to obtain that which we all feel ought to be obtained, the production of rotary motion from steam at first hand instead of the obtaining it at second hand through reciprocating motion. I may say that this science collection offers very little inducement indeed for me to speak to you about rotary engines, for so far as I know it contains but three specimens. Here, in one by the late Patrick Bell, we have a cylinder bent into a semi-ring, with a continuous slot on its inner side; a disc, working steam-tight in this slot, carries pistons which you will see are intended to enter and to leave the cylinder edgeways, but while in the cylinder to lie flatways, so as to be pressed upon by the steam. I do not know that this engine was ever more than an idea. I do not know the date of it, and I do not think any of us will look at it as likely to give economical results. Here is another machine which has been commonly classed as a rotary engine, but I am not sure that it is correctly so classed; it is the celebrated Disc engine, from which so much was expected at one time. It is in truth a reciprocating engine because the disc reciprocates from side to side, changing the point of contact, and by means of a rod standing out at one of its faces gives rotary motion to the crank. After all there is a crank. The crank is a necessity, and therefore such engines can hardly be classed among the rotaries. The third model I have here is little more than a Barker's mill.

There is no doubt that many of the persons who set themselves to devise rotary engines, do so, because they are under the impression that there is an absolute loss of power by the use of the crank. I need not tell gentlemen who are here to-day, science teachers, that there is no loss of power at all theoretically speaking. As a matter of fact you may by the friction of the shafting and its bearings, by the necessity of

having a fly-wheel and matters of that kind, turn a certain amount of power into heat in your bearings, which had better be employed in driving the machinery, and therefore and also for the purpose of simplicity, if it could be attained, the rotary engine is a thing to be desired. But, commonly speaking, the efforts to obtain a simple engine, end in disappointment, and in much greater complication than prevails in the machines which they are intended to supersede.

Having made these few remarks on the history of our subject (which remarks have carried me much further than I had intended), I will now ask you to consider with me what are the points to be borne in mind in constructing an economical steam-engine. In the expression "steam-engine," I think I must (although want of time tempts me to do otherwise) include the boiler, that very essential element. Were it not so essential, I would ask you to suppose that by some means we had obtained steam economically, and that the object of the Lecture was to consider how far that steam so obtained could be used with advantage. But I think probably it will be as well that we should devote some little time to the boiler itself. As to the kind of boiler to be used, the first thing to be considered is safety; the next things in very many instances indeed are weight and compactness. When we have, for example, to consider the boiler of a common road locomotive like Hancock's, of which I have a diagram on the wall, or when we have to consider the boiler of a locomotive or the boiler of an ocean steamer occupying space which might be filled by cargo paying freight, or when we have to consider boilers put into buildings in the city of London where land lets for a guinea a square foot per annum, we may readily see that the compactness of the boiler is an element which weighs very materially in its construction. There are, however, many cases for other purposes, such as water-works, large cotton-mills, and establishments of that kind where the engine constructor is at liberty to give the boiler any size and weight he thinks necessary, so that occasionally we have to take into consideration the question of compactness, and occasionally it is a matter of comparative indifference. In old days (which, I am sorry to say, means when I was an apprentice) the kind of boiler commonly in use for land engines was the waggon boiler, a boiler set over the fire, the products of which went under the boiler bottom to the end, and origi-

in this time. If I deliver in the same time 8 lbs. weight of steam from the same surface of water under a pressure of eight atmospheres, those bubbles would be only as numerous and as large as were the bubbles of the 1 lb. of steam escaping from the water at the pressure of one atmosphere, because the steam being eight times as dense it is obvious that you get it off in a more compact form, and that, therefore, the mechanical agitation of the water is no greater under those circumstances than it would be with the lower pressure and the smaller quantity of steam, and that this is so is practically known to every intelligent man who has the conduct of a steam-engine. Such a man is well aware that if a boiler primes, the way in which he can check it is by partially closing the regulator so as to keep the pressure up. We equally know that if the pressure suddenly falls in a boiler from any cause, such as the blowing away of a safety-valve, every drop of water comes out of the boiler with the rush of steam, the fact being, that tranquillity at the surface, which tranquillity is necessary to prevent priming, is to be obtained by proportioning the ratio of the water surface to the number and size of the bubbles of steam (no matter what their density) which have to come through it in a given time.

The foregoing has been a digression. I will now ask you to consider the question of boiler surface. In the diagram *A* on the wall with its single flue representing the Cornish boiler, let us consider for the present the flue surface only. Compare with it diagram *B* where the single flue is replaced by seven flues each $\frac{1}{3}$ the diameter of the large one in *A*. It is perfectly certain, that by that means we have $2\frac{1}{3}$ times the surface in *B* we had in *A*. Then you will see if you divide each one of the seven tubes into seven again, you get $2\frac{1}{3}$ times $2\frac{1}{3}$ which equals $5\frac{4}{9}$ the original surface. In that way you might say to me, if you were not engineers, why not carry through to infinity and get an infinite surface in the boiler? There is a very good answer to it; in fact several answers, any one of which is good. One is that if you have too much surface in the boiler in proportion to the steam to be delivered, you do not have a sufficiency of water between the tubes to insure that there shall always be water in contact with them. Another is that you do not afford a passage between the tubes for the steam generated by the lower tubes to get clearly away to the

water surface. Another is that when the water is foul, the tubes become so coated with deposit as to fill up the spaces between them almost to a solid, and you cannot clean them. But another and a very important reason is this—through these tubes have to go the products of combustion. Those products commonly have not completed their combustion in the fire-box, but you require it to be completed in the tubes; and if they are too small you have made your boiler into the meshes of a Davy lamp, and by means of these tubes you have as effectually snuffed out and extinguished the gases which go through them, as the meshes of a Davy lamp extinguish the inflammable gases which endeavour to pass through them. You therefore have very good reasons, all of them, why the multitubular system should not be carried too far. In practice in steam-boats the tubes are generally from $2\frac{1}{2}$ to $3\frac{1}{2}$ inches in diameter, with probably from $\frac{7}{8}$ to $1\frac{1}{2}$ inches of water space and with a length of tube of 6 ft. 6 in. In locomotives, where you have a forced draft and therefore do not require so large an area to get the products of combustion through, the tubes average probably from $1\frac{1}{4}$ to $1\frac{3}{4}$ inches diameter, and their length varies from 8 to 10 feet. The tubes of the portable agricultural engine lie between these dimensions. To-morrow I hope to say a word to you about the proportion of surface, but in speaking of the actual proportion I am inclined to state that, as far as my experience goes, it matters very little indeed, except upon the scores of safety and ability to clean, and of having a large water surface to deliver steam, what kind of boiler you employ, so long as it has an adequate absorbing surface for the heat evolved from the fuel in a given time. I believe the various contrivances or notions about vertical surfaces and flat surfaces, and all these things which have agitated engineers' minds for many years, are absolutely unimportant so long as you have an adequate absorbing surface for the heat produced. It is much more important to have a construction which will afford dry steam, power of cleaning, and of repair. I will now ask you to consider a little how much of a successful economic result is due to the proportioning of the boilers, and to good stoking. On the wall is a table, which is only a small portion of one published by the Royal Agricultural Society relating to the performance of portable engines under trial by that Society. You will find two columns showing the number of pounds of

water actually evaporated by 1 lb. of Llangennech coal, assuming the water to be at 212° which it was very nearly. The other column shows the water that could have been evaporated had the feed water been at the ordinary temperature of 62°. We will confine our attention to the first column, and you will see there that the average obtained was 9·85 lbs. of water boiled off by 1 lb. of coal. This column is sufficient to show the extreme danger of judging by an average unless you know the maximum and the minimum, for if you look at the second line from the top you will find that as much as 11·83 was boiled off in one case, while in another case it was only 4·84. The same coal was used; the difference was in the proportions of the boiler, and in the ability of the stoker. With respect to the stoking on these trial occasions, the stoking is done by men, some of whom are paid several hundreds a year. It is done by a hand fire-shovel, more like a banker's scoop than an ordinary shovel; the coal is broken into bits about the size of a walnut, the fire-door is opened about once in every two minutes (which you will find very wrong, according to Tredgold, and so it would be if the men were not trained to do it by sleight of hand so dexterous that it is hardly possible to see the operation performed); and in that way there is kept up an absolute uniformity of combustion, a uniformity so great that, during the five hours which the trial sometimes lasts, I will undertake to say the pressure gauge does not vary one-sixth of a pound. So accurate is the firing that whereas the engine would blow off with 80 lbs. and thus waste steam which would spoil all the competitor's chance, at 77½ lbs. the engine could not run against the weight, therefore the stoker's margin of possible working in these engines lies within 2½ lbs. Such accuracy you cannot hope to obtain in daily work. Let us see how essential good stoking is. What is it that we want to do? We want the most perfect combustion of the fuel, coupled with the least admixture of air, for the purpose of making that combustion. Assume for the sake of illustration that every pound of coal gives us 14,000 English heat units if we burn it to carbonic acid, and that we only get in round numbers 4,000 units if we burn it to carbonic oxide, that is to say, in the latter case we only get $\frac{4}{14}$ and lose $\frac{10}{14}$ of the effect. What we have to insure, therefore, is, that sufficient air is introduced into the furnace

to consume the whole of the fuel and convert it into carbonic acid, and that none for want of air shall escape out of the fire in the condition of carbonic oxide, but, on the other hand, every particle of air which enters the fire-grate beyond the quantity required is a source of loss by increasing the weight of heated material which escapes unutilized up the chimney. It is a common thing for newspaper writers to point out that smoke is not only a nuisance, but is a waste of fuel which might be burnt. So it is, but it is far less waste than would go on if you had furnaces so managed as to pour up the chimney carbonic oxide. This being invisible does not excite attention, and thus two men might be employing fuel; one sending up no visible smoke (although plenty of carbonic oxide), would receive commendation as an economical man, while the other, who did make smoke (which he ought not to do), but made no carbonic oxide, would be improperly stigmatised as the extravagant man. I believe what we have to do is to see if we can obtain mechanical stoking, and on this point I should like to be allowed to say a few words on combustion generally. It appears to me that you may use fuel either in the form of gas, as is practised by Dr. Siemens; in the form of liquid, as in the use of hydrocarbons; in the form of almost impalpable powder as used by Mr. Crampton, or in the ordinary form in which we get it as coal, and as we employ it in steam-engines. If you employ it as coal, then clearly the only portions which are available for combustion at one time are the surfaces. In order to develop sufficient heat for your purpose, you are compelled to have within the fire-box, or on the fire-grate, as much coal or coke as will when you take its surface into account (if you could measure it), unite with enough oxygen to produce the required heat in a given time; and for this to be done you must either have a large extent of grate, or must keep the fire open, or must have a powerful draft; with a large extent of grate, there is the difficulty that it should at all times be covered, otherwise air runs freely through the spaces uncovered by fuel, causing much loss. On the other hand, with a great depth of fire you have the very difficulty I have been pointing out—that of making carbonic oxide. Carbonic acid is produced in the lower part of the fire, and this carbonic acid rising through the hot fuel above, takes up another

atom of carbon, and goes out as carbonic oxide. There are these great difficulties in actual manual firing, and it is to get over them that the engine-driver at the Royal Agricultural trials fires once in every two minutes, and obtains practical uniformity from the beginning to the end of the five hours' trial. But, as I say, this is impossible in practice. When fuel is dealt with in other ways it is far more manageable. It is all surface, if I may say so, when it is reduced to impalpable powder, because the surface of a given weight in that form may be popularly said to be infinite as compared with the surface of lumps of coal, and therefore when working on this system you can properly proportion the supply of powder and of air to each other, and so obtain extremely good results by this mode of firing. At present I am sorry to say that Mr. Crampton is not helping us in the economic generation of steam, for he is devoting himself to the application of his system not to the obtaining heat for steam-engines but to the production of the high temperatures required in the puddling of iron. In the case of the liquid hydrocarbons, and in that where the fuel is converted into gas, as in Dr. Siemens' producers, it is also possible in practice to regulate with nicety the amount of the air, and it is from this power of regulation that the large percentage of useful effect has been got from liquid fuel, and that the high results arising from the combustion of gas have been got even when that combustion is unaccompanied by the regenerative system of Dr. Siemens, which system has caused the vast economy of fuel which has been obtained since its introduction in many of our most important metallurgical and other processes.

ON THE STEAM-ENGINE.

LECTURE II.

Reverting to the subject of boilers, I did not yesterday mention the kind of boiler which is in most general use now-a-days in England for manufacturing purposes—I mean the Lancashire boiler, the internally fired two-flued boiler. It probably is one of the very best that we have from a practical point of view, as it gives in a comparatively small diameter a large area of fire grate, and it gives also the considerable heating surface of the internal tubes. Moreover it affords ample opportunity for getting in below the tubes for the purposes of cleaning or of repairs when necessary. The marine boiler of the present day is, as we know, a boiler from 11 to 12 or 13 feet in diameter, made of plates up to an inch in thickness, and competent to stand a pressure of steam of 70 lbs. above atmosphere. It contains generally three cylindrical fire-tubes, the flame-box, the return-tubes, and a dry uptake. The marine boiler of old days was, in truth, nothing but a tank, the form of which was determined by consideration as to its adaptability to the shape of the vessel more than by any other rule. The flues were rectangular and flat-sided, and it was wholly unfit for use except with low pressure. To show you how low the pressure was, I may state that there was more pressure on the bottom of such a boiler (considered merely as a tank) from the weight of water in it (when the steam was not up) than was pressing against the under side of the top of the boiler when steam was up.

The cylindrical cases of boilers resist the tendency of the internal pressures to burst them by means of the tensile strength of the plates and of the riveted joints; and flat surfaces, where they must exist, such as at the ends of cylindrical boilers, are stayed either by stay-bars or by gussets; or, in the case of the fire-boxes of locomotives, by screw stays $4\frac{1}{4}$ inches apart and upwards; but there is

another set of circumstances to be considered, namely, that when cylindrical flues are exposed to external pressure—and I think we owe it to the late Sir William Fairbairn that that which was formerly a most fruitful source of giving way in steam-boilers has been cured, because, although a perfect cylinder subjected to pressure ought to bear more than it would now, the pressure applied as an internal bursting strain, yet even a perfect cylinder when pressed from the outside is in a state of unstable equilibrium, and, in practice, a flue thus pressed, therefore, is subject to being destroyed at almost any moment, either by its not having been originally a perfect cylinder, or by differences of temperature, or by wear; and we know as a fact that such flues collapsed under pressure, and thus they were a constant source of accident in internally fired boilers. Now this defect has been entirely cured by the expedient of riveting at intervals T iron, or a specially made sort of channel iron, round about the flues, but there is one very simple mode applicable to all existing boilers which I think is equally efficacious with the riveting system, and is better in other respects, as there are no rivets to be acted upon by the fire. That mode consists in simply cutting out of a piece of boiler-plate a ring (which if it be in an existing boiler may be made in halves and bolted together), then if that ring be slid on over the tube so as to fit it, the edge of the ring where it touches being bevelled off, in order that there may be as little a part of the tube cut off from water contact as possible, it will be found, although there is no attachment between the ring and the tube, that you have most effectually prevented the tube from collapsing, for the simple reason that it cannot diminish its diameter in one direction without correspondingly increasing it in another. In that way without any rivet or attachment, it is possible to make flues of Lancashire and Cornish boilers safe against collapsing.

The only other kind of boiler I will mention to you is one which was in use a great many years ago, and was called the "Quicksilver-boiler." It was employed on a large scale (that is to say on a fair working scale), to drive the mill engine of the King and Queen Ironworks, the property of Mr. Howard, the inventor, also on a paddle-steamer called the "Vesta" which ran as a trading boat to Ramsgate. This boiler had a double bottom to it. The upper one (the false bottom)

having cups projecting downwards to add to the surface, and between the two bottoms, and surrounding the cups there was an amalgam of quicksilver and lead that was kept at a temperature of, I think, about 450°. There was no water whatever in the boiler, and when it was desired to start the engine, the attendant, by means of a hand-pump, injected as much water on to the cup surface as would generate sufficient steam for a half revolution of the engine. The feed-pumps of the engine were forced inwards not by eccentrics, but by springs, and the amount of their inward stroke could thereby be regulated to a nicety by a set screw, and in that way the engine ran without there being at any time an appreciable quantity of water in the boiler, the stored up heat between each injection of water accumulating in the amalgam. This steamboat ran for very many months, and the factory engine, I believe, for very many years. The government tried it in one of their boats, but eventually it was abandoned, and I merely mention it to you as an interesting link in the history of the steam-engine.

With respect to the staying of boilers, there is one other boiler I must mention, and that is the boiler of Hancock's common-road locomotive to which I alluded yesterday. This was composed of a succession of flat chambers ranged side by side like books on a shelf, the spaces between the chambers being about one inch, and the chambers themselves being about 1½ inches wide, and, according to the size of the boiler, from 20 inches to 30 inches square or broad and high. These chambers thus arranged side by side, were put over the fire which played between them in the spaces. The water was contained in the lower part of the chambers, and the steam at the top, and I think we shall all agree that a more impossible form for withstanding high-pressure steam, it would be difficult to imagine—that is, at first sight. But each of these chambers had raised upon it hemispheres, and the hemisphere on the side of one chamber abutted against the corresponding ones on the side of the next chamber, and thus the chambers, say to the number of fourteen, stood side by side, each chamber supporting its neighbour, and there remained only to be stayed the two outside plates of the outside chambers. This was done by means of thick boiler plates which pressed on the hemispheres of those outer chambers, and those outer chambers and plates were held together by outer tie-rods and cross

another set of circumstances to be considered, namely, that when cylindrical flues are exposed to external pressure—and I think we owe it to the late Sir William Fairbairn that that which was formerly a most fruitful source of giving way in steam-boilers has been cured, because, although a perfect cylinder subjected to pressure ought to bear more than it would now, the pressure applied as an internal bursting strain, yet even a perfect cylinder when pressed from the outside is in a state of unstable equilibrium, and, in practice, a flue thus pressed, therefore, is subject to being destroyed at almost any moment, either by its not having been originally a perfect cylinder, or by differences of temperature, or by wear; and we know as a fact that such flues collapsed under pressure, and thus they were a constant source of accident in internally fired boilers. Now this defect has been entirely cured by the expedient of riveting at intervals T iron, or a specially made sort of channel iron, round about the flues, but there is one very simple mode applicable to all existing boilers which I think is equally efficacious with the riveting system, and is better in other respects, as there are no rivets to be acted upon by the fire. That mode consists in simply cutting out of a piece of boiler-plate a ring (which if it be in an existing boiler may be made in halves and bolted together), then if that ring be slid on over the tube so as to fit it, the edge of the ring where it touches being bevelled off, in order that there may be as little a part of the tube cut off from water contact as possible, it will be found, although there is no attachment between the ring and the tube, that you have most effectually prevented the tube from collapsing, for the simple reason that it cannot diminish its diameter in one direction without correspondingly increasing it in another. In that way without any rivet or attachment, it is possible to make flues of Lancashire and Cornish boilers safe against collapsing.

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girders. There were also two inner tie-rods, one at the top, and one at the bottom, which, passing through all the chambers and through certain washers, served at the same time as tie-rods, and by the annular spaces between the rods and the insides of the washers as a steam-pipe at the top and a feed pipe at the bottom. The plates were not above $\frac{1}{8}$ of an inch thick, and the pressure of steam was, I believe, as much as 80 lbs., and those boilers were the means whereby Hancock was enabled upwards of thirty years ago to drive his carriages through the streets of London, and to take passengers from the bank to Paddington and to Whitechapel regularly amongst the omnibus traffic without hindrance of any kind or description. There were various meritorious points in that steam-carriage, which I should like to go into if time permitted. Up to the present it has not only not been surpassed, but I am sorry to say that particular branch of engineering has gone backwards, and up to the present time Hancock's carriage has not been equalled. It was noiseless, smokeless, sightly, easily steered, light and powerful, necessary qualities all for street navigation and for avoiding the alarming of horses. Hancock attended to all these points, while the engineer of the present day, although he may make a very good engine, does not appear to consider that he has to make one which is to go about amongst horse traffic.

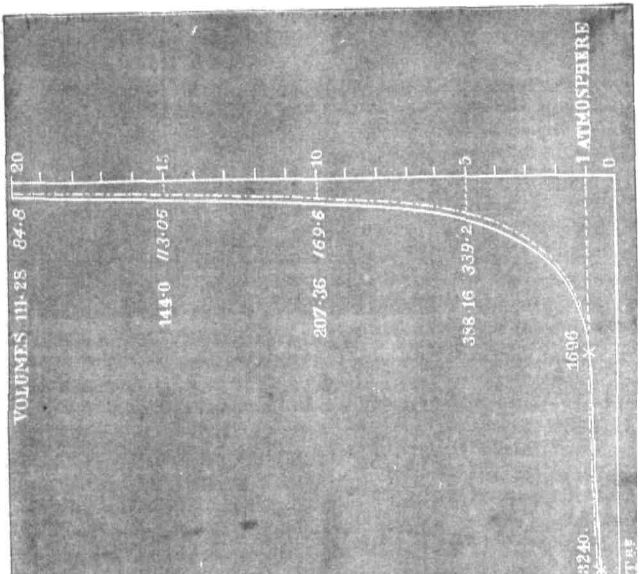
With respect to the proportion of boilers, in the table that I exhibit here of the engines tried at the Royal Agricultural Society, I may tell you that the proportion of absorbing surface of the engines varied from 16 superficial feet per cubic foot of water evaporated per hour, to 37 superficial feet. With the 16 feet there was obtained ·651 of the total heat effect of the coal, and the gases escaped at 775°. With the 37 feet super there was obtained ·776 of the total effect of coal, and the gases escaped at 500°. I may say in the face of such discrepancies in proportion as those, it is difficult to fix on any rule, but the ordinary rule is from 12 to 15 superficial feet of heating surface for each cubic foot of water evaporated per hour, while the surface of grate where there is no forced draft is from $\frac{1}{24}$th to $\frac{1}{80}$th of the heating surface.

Before leaving the subject of boilers I should like to call your attention to the solar boiler which is being worked at Tours; an account of this boiler is to be found in the *Revue*

Industrielle for the 24th of October last. I dare say the particulars are known to most of you, but as they may not be to all, I will repeat them. The effect of the solar rays on 45 feet of surface when concentrated upon the boiler was sufficient to evaporate 11 lbs. of water in one hour. The construction was simply this. A conical mirror was employed, the sides of the cone being at an angle of 45°; the diameter of the cone was 2·6 metres, and the bottom of it had a flat surface of one metre. In the axis of this cone was a copper boiler about 11 inches in diameter, and of the height of the cone. The boiler was blackened, and was surrounded by a glass envelope; there was also an apparatus to cause the axis of the cone to point always towards the sun, and the sun's rays falling upon the sides of the mirror were reflected radially on to the boiler, and the result was, as I have said, to obtain an evaporation of as much as 11 lbs. of water in one hour from the effect of the sun's rays falling on 45 feet super.

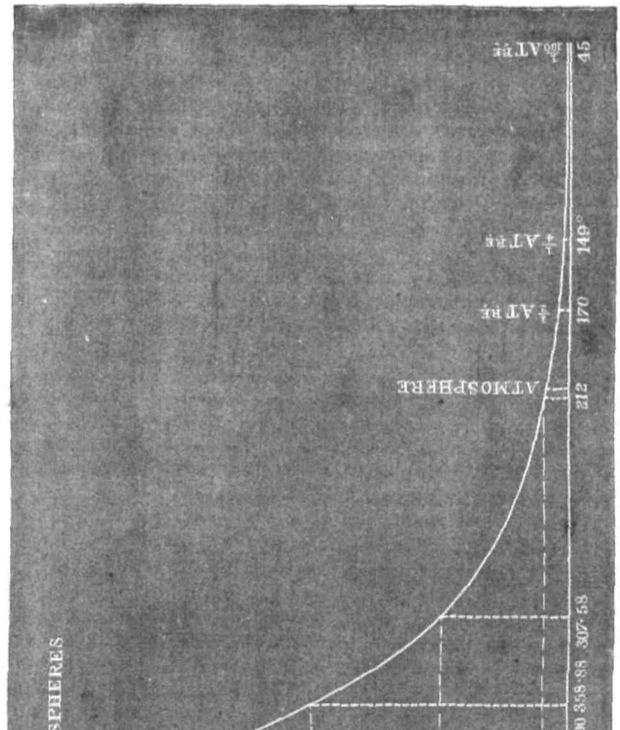
We frequently hear of very wonderful evaporating performances, and when they are very wonderful we should do well to distrust them. We must recollect that one of the defects of a boiler is priming, and we must equally recollect that that abstracts water from the boiler, and, therefore, appears to make the boiler do higher evaporating duty, and a bad boiler is thus credited with a merit for that which is a positive defect. I know no means of making quite sure that this priming does not take place, except by recondensing the steam that comes from the boiler and observing the heat given forth by it; but by this precaution it may soon be ascertained whether the product from the boiler has been dry steam, or partially steam and partially water mechanically carried over.

Assuming that we have once got from some boiler the steam, we will now consider how we are going to use it in the steam-engine, and I will refer you to these two diagrams —one where I show by a vertical line the pressure of the steam from zero up to 20 atmospheres, and by the horizontal line the temperatures; the other (2), showing the volumes corresponding to the various pressures.



VOLUMES 111.28 84.8
144.0 113.05
207.36 169.6
388.16 339.2
1696
3392 3240
6784 6161
20
15
10
5
1 ATMOSPHERE
0

Now having the steam, there are four modes in which we may use it in an ordinary reciprocating steam-engine. We



may use it non-condensing and non-expansive; condensing and non-expansive; non-condensing and expansive and con-

densing and expansive. I will first of all take the simplest and worst form of steam-engine, that of non-condensing and non-expansive, and I will just ask you to consider how important it is under these circumstances that the steam which is used should be of very high pressure. So obvious is this that it would seem absolutely unnecessary to labour the point, were it not that one knows in practice steam-engine users, and too frequently steam-engine makers, will employ large cylinders to perform a given amount of work in non-condensing, non-expansive engines, having a notion that they are really doing good, and are at the same time acting liberally, whereas they are doing harm and are making the engine wasteful. The effects in a non-condensing and non-expansive engine of using different pressures of steam are shown in Diagram 3. I will assume, to begin with, that we have a pressure of steam in the boiler 120 lbs. above zero, and for simplicity and to get rid of fractions I will take the atmosphere as equal to 15 lbs. At 120 lbs. above zero if we use a steam-engine without expansion and deduct 15 lbs. for the resistance of the atmosphere we get from a given quantity of steam a work of 105. If we use it at only 60 lbs. above atmosphere, deducting 15 lbs., we obtain a work of 45, while if we use it at 30, and deduct 15, we obtain a work of 15. If, however, we use it at 18 above atmosphere and deduct 15 we obtain a work of only 3; that is to say, the percentage of coal required to raise the steam to atmospheric pressure is clearly greater the less is the pressure of the steam. If we try to work a non-condensing engine with steam at atmospheric pressure, evidently, we should get no work out of it at all, although we should expend the coal in boiling off the water; if the engine were worked at 1 lb. above the atmosphere, $\frac{15}{16}$ths of the coal would go away in boiling the water and $\frac{1}{16}$th only would be utilised. But if worked at 120 lbs. above zero or 105 lbs. above atmosphere, $\frac{1}{8}$th only of coal is lost, and $\frac{7}{8}$ths are utilised. Therefore we see how essential it is that to obtain in a non-condensing, non-expansive engine anything approaching to economy, high pressure steam should be employed.

I will now ask your attention to the advantage in non-condensing engines of the use of expansion. The results are expressed on this diagram, which exhibits the well-known

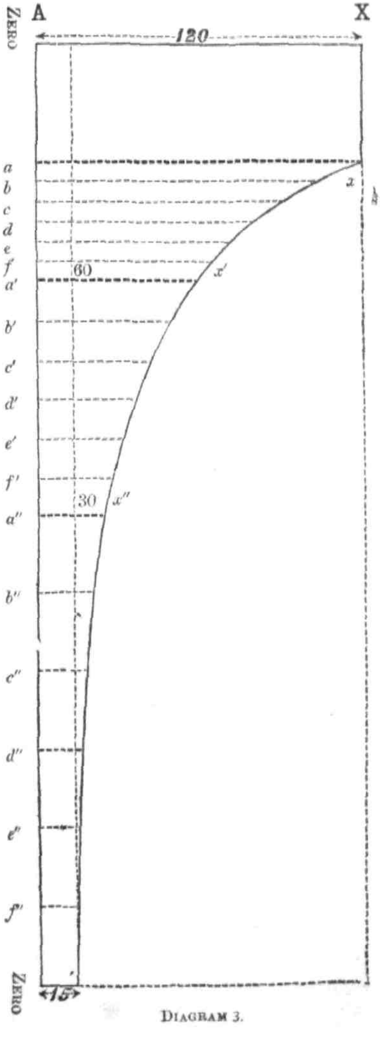


DIAGRAM 3.

hyperbolic expansion curve of steam, taken on the assumption that the volumes are inversely as the pressures. Supposing we once more begin with the pressure of 120 lbs. above zero and let the steam in through a space of one, viz., A to *a*, then shut it off and expand it through an equal space (*a* to *a'*), we shall obtain a pressure of 60 lbs. above zero as the final pressure, and obviously we should have got in addition to the work represented on the upper part of the diagram by the parallelogram (A X *a x*) of the steam let in, the work represented by the part next below it, this figure (*a x a' x'*), the area of which is all but $\frac{7}{10}$ths of the area of the top figure. Now if one expands that steam which is at this time of the capacity of 2 into the capacity of 4 (*a'' x''* line), it is quite clear that the area contained between the line *a' x'* and the *a'' x''* (the steam being now 30lbs., above zero pressure finally), is exactly the same as that of the previous figure, because while every one of the horizontal lines, *b' c'*, &c., is half the length of the horizontal lines, *b c* corresponding to it in the figure above, every one of the vertical lines is double the length of the similar vertical line above, and therefore the area is the same, and thus by this further expansion we have got nearly a further $\frac{7}{10}$ths of the original pressure. Again, if we pursue that and double the volume we get a further $\frac{7}{10}$ths and it would appear on the face of it, as if one might go on *ad infinitum* and get infinitely great power. That of course cannot be true. It would be at once seen it was not so if, instead of taking a curve varying inversely with the pressures, we had made our lines after allowance for the difference of temperature; then the true expansion curve thus obtained would have been found to be one that could not be infinitely pursued. But even without taking that into consideration it would not pay to carry the expansion curve down to a very great extent, because the size of the machinery would become so enormous; and therefore, one is compelled for commercial considerations to keep the expansion within certain reasonable limits.

Diagram 4 shows, in a tabular form, the value of expansion when non-condensing. Supposing in lieu of using a cylinder full of steam at 120 lbs. the steam is cut off at half stroke, it is quite clear that we have only used half the steam, and the work which we shall get in that part before cuttingoff will be of the value of 60, but in the part after the cut off we shall get

NON-CONDENSING EXPANSIVE ENGINE.

Steam cut off ½ stroke ...	1 × ½ = ½	½ × 120 = 60	60 In part before cutting off.
			41½ ,, after ,,
			101½ Gross.
Atmo. resistance	1 × 1 = 1	1 × 15 = 15	15 Back pressure.
			86½ Net work done.

Thus half the weight of steam has given $\frac{86\frac{1}{2}}{105} = 82{\cdot}5\ \%$

Or the full ,, ,, would give ... 165

DIAGRAM 4.

NON-EXPANSIVE CONDENSING ENGINE.

	Area.		Stroke.		Contents.	Pres. wt. of steam used.	
Full Steam	1	×	1	=	1	1 × 120 = 120	120 gross work done.
Back pressure	1	×	1	=	1	1 × 2	2 Back pressure in condenser.
							118 Net work done.

Diagram 6.

AIN PER CENT. FROM THE USE OF CONDENSATION IN A NON-EXPANSIVE CONDENSING ENGINE AS COMPARED WITH A NON-EXPANSIVE NON-CONDENSING ENGINE.

Pressure of steam above zero.	Pressure of steam above atmo.	Net work obtained without condensation.	Net work obtained with condensation.	Percentage of gain.
120	105	105	118	12·5
60	45	45	58	28·8
30	15	15	28	86·6
18	5	3	16	433·0

DIAGRAM 7.

IN PER CENT. FROM THE USE OF CONDENSATION IN AN EXPANSIVE CONDENSING ENGINE AS COMPARED WITH AN EXPANSIVE NON-CONDENSING ENGINE.

eam pressure above zero.	Cut off.	Wt. of steam used in cylinder of capacity 1.	Useful effect, non-condensing.	Useful effect, condensing.	Percentage of gain.
120		60	86½	99½	15
120	¼	30	56⅓	69⅓	23
120	⅛	15	31	44	42

DIAGRAM 8.

still get an effect of 31 without condensation, or with condensation 44, and the gain per cent. therefore is 42.

No. 9 is a very similar Diagram, showing you the effect of using 120 lbs. steam with condensation, but without expansion, and also where you expand that steam twice 4 times, 8 times, 16 times, and 32 times, the net useful effects being 118, 199, 277, 352, $418\frac{2}{3}$, $468\frac{2}{3}$, and the ratio 1, 1·68, 2·34, 3, 3·54, 3·89.

One sees, therefore, that for economy, condensation is a most necessary thing. More especially when the pressures are low, condensation really becomes a duty. There are various means of effecting it. There is the old injection condenser invented by Watt, where the stream of water goes into a chamber and the condensed steam and the liberated air are withdrawn by means of an air-pump. This requires a constant supply of cold water, and that cannot always be obtained in sufficient quantities. Then we have to resort to means of recooling. One of the commonest is, as we know, a cooling pond, but that requires a considerable amount of space. Another one is a pile of brushwood, down which the water is allowed to trickle. In both those cases the loss by evaporation equals the feed water. Another method was that pursued by Howard to be used with his quicksilver boiler. He passed the injection water through pipes which were outside the hull of the vessel, and which were acted upon by the sea-water, and in that way he recooled the injection water and used the same water over and over again. It was a necessity for him to have pure water, because with the boiler I have described a deposit upon the surface above the amalgam if at all serious would very rapidly have caused the boiler to burn out. Then we have the surface condensers, of which we found a model yesterday in Mr. Watt's case. This condenser is largely, in fact almost universally employed now-a-days in marine engines. The proportions of it vary very much according to the rapidity with which the water is driven through, and according also to the temperature at which the water is expected to be found. A steam-boat intended to navigate the Red Sea should be provided with a much larger surface in her condenser than one intended for the Atlantic. There is a very elaborate paper by a French engineer, an abstract of which has been translated and will be found in the *Proceedings* of the Civil Engineers, which

THEORETICAL INCREASE IN USEFUL EFFECT ARISING FROM THE EMPLOYMENT OF VARYING DEGREES OF EXPANSION IN A CONDENSING ENGINE HAVING A BACK PRESSURE OF 2lb.

Weight of steam used.	Expansion.	Net useful effect.	Ratio.
120	0	118	1·00
120	2	199	1·68
120	4	277	2·34
120	8	352	3·00
120	16	$418\frac{2}{3}$	3·54
120	32	$468\frac{2}{3}$	3·89

DIAGRAM 9.

by formulæ shows how you may vary the proportion according to the amount and to the temperature of the water pumped past the cooling surfaces.

The before-mentioned condensers, whether injection or surface, require a large amount of water, but there is another kind of condenser not much used, but which should be a great deal more used than it is, for the purpose of surface condensing, a condenser by means of which you can, wherever you have sufficient water to feed a boiler, efficiently condense the steam of the engine. This condenser consists of a number of rows of horizontal tubes connected at the ends to vertical stand-pipes. Over the topmost of these rows there are troughs, perforated, into which the condensing water is pumped, and from which it trickles over the surface of the condenser and thereby condenses the steam, there being evaporated from the outside of the condenser in the form of mist or reek almost exactly the same weight of water as was used for the feed. The condensing water that is not evaporated is collected at the bottom and is repumped in a warm state on to the top of the condenser, and by that apparatus it is, as I have said, perfectly practicable to maintain a very excellent vacuum of some twenty-four or twenty-six inches by an expenditure of no more water than that which of necessity must be expended in the feed of a non-condensing engine.

Besides that we have had condensation effected by means of the air. A good many years ago a gentleman of the name of Craddock invented an air condenser which consisted of a cage placed on the roof of the engine-house and joined by a stuffing-box to the eduction-pipe. This case was caused to revolve in the air, and the contact of the air with the surface effected condensation. I have not the particulars, and I do not think the vacuum obtained was very good, but nevertheless there was a vacuum obtained, and the condenser was put to work at the London works, Birmingham, then the property of Messrs. Fox, Henderson, and Co. Then for the purpose of bringing the steam back into water to be used over again we have the air condenser of Hancock, who, in his engines, had the condenser made on the same plan as his boiler, through the condenser the air was blown, and that air afterwards fed the fire.

There are very many benefits attending the use of surface

condensers and evaporative surface condensers (apart from the mere obtaining a vacuum) in substitution of a jet condenser. Among the benefits are, that you get pure water for the boiler, you do not get the boiler fouled by a deposit. On the other hand, you must take care that you do not get the water too pure, because if you have not some kind of scale on the plate, as there is always a certain amount of fat acid which gets into the boiler from the grease, the plates "pit" and rapidly decay. But if you let in just as much new water as is necessary to make up for the waste that inevitably takes place, that, generally speaking, deposits sufficient impurity to prevent the condensed steam acting on the bare plates. In surface condensation in the marine engine you at once get rid of the necessity for blowing off a certain portion of the heated water to discharge the brine from the boilers; I mean the continuous blow off that was employed when injection condensers were used and where you put into the boiler about double the necessary feed water in order that (the water in the boiler being maintained at twice the saltness, if I may use the term, of that of the sea,) one-half being blown away it may carry with it two particles of salt, the two particles that came in with the double quantity of water. This continuous blow off involved the waste of a great portion of the heat, but this loss has been obviated altogether by the use of the surface condenser. Another advantage is, that we get rid of the presence of air in the fresh injection of water that comes in, in the ordinary jet condenser.

I have said that in the injection condenser the water is generally abstracted by an air-pump, but we know that it has also been abstracted by means of a pendant pipe of such a length that the pressure of the atmosphere cannot balance the column of water within it, and we also know that for some years past Mr. Morton has employed what is called an ejector condenser, not only for extracting the water and air, but to act as the condenser itself. In engines running at any considerable speed, especially if coupled engines, where there is a sufficiently rapid succession of beats of the exhaust steam, it is possible without the use of any live steam from the boiler and by the means of the exhaust steam, and the jet of water coming in, to cause the products of condensation to pass out of an ejector condenser against the pressure of the atmosphere, carrying with them the air: one

of these condensers was put to work in the building below us about two years ago, applied to a high-pressure engine driving the machinery in the Exhibition. The ejector condenser is an extremely simple apparatus and one readily applied to any existing engine, and there are in it no working parts. I will not endeavour to describe the principle of its action, because if time permits I propose to repeat the explanation I have given elsewhere of the principle of Giffard's injector, and this explanation is applicable to the ejector condenser.

About the earliest instance we have of the expansion engine doing admirable duty, is the Cornish pumping-engine, which is shown in diagrams 19 and 19a. You are aware that those engines are single-acting, that the steam comes in at the top side of the piston, the vacuous condition being below; after the piston has gone down a certain distance, the steam-valve is closed by the action of the plugtree and the rest of the journey is performed by expansion. When applied to a mine the engine lifts the weight of the pump-rods, and on the return journey, the equilibrium-valve being open, the weight of those rods acting on the plunger-pump raises the water up to the surface of the ground. In a mine, therefore, the resistance to this engine is constant, because the depth of the mine being constant the head of water against the pump is constant. Those engines were admirable machines in their day, and they developed an economy which prior to that time had been thought to be impossible. Their performance is commonly expressed, as you know, in millions of duty, that is to say, in millions of pounds raised one foot high by the combustion of 112 pounds of coal. Before the year 1855, the record used to be in millions of pounds raised one foot high by the combustion of a bushel of coal, which was taken as 94 pounds. You are probably aware that in 1811, Mr. Lean, a Cornish engineer, the inventor of the plunger-pump, established Lean's Reporter. He commenced, I think, with three engines in the counties of Cornwall and Devon, and in his Reporter he gave month by month the performances of the respective engines. A great deal of emulation was caused thereby, and there is no doubt that the establishment of that "Reporter" was a great incentive to engineers to improve the quality of their engines. The conversion of millions of pounds duty into

horse-power is one which we ought to have very readily in mind, because we are apt to be led away by the large figures of *millions*; it is something like a man's income being stated in francs; we want a ready method of converting the francs into sovereigns. I have put here a table showing this conversion: one pound of coal per horse-power per hour would equal $221\frac{3}{4}$ millions of duty of the present day. It is easy to remember that one pound of coal per horse power per hour is equal in round numbers to 222 millions of duty, and that 2 pounds of coal per horse-power per hour are equal to 111 millions of duty, and so on. I had occasion to speak yesterday of the difficulty even among engineers of getting out of a fashion. The case of the Cornish engine is a further illustration of this; the Cornish engines were very good in Cornish mines, and thereupon Cornish engines were introduced into waterworks. Now except for the purpose of pumping up into a reservoir, where the head of water being nearly constant, the engine is in the same condition as it is in when pumping water to the surface of the ground from a mine, I am prepared to give my opinion that the employment of Cornish engines for waterworks purposes is an entire mistake. They operate by the descent of the weight forcing up the water, and that weight being constant must always be balanced by a constant pressure. To obtain this, what the waterworks' engineer does when he employs a Cornish engine to pump into the distributing mains, is to put up a stand-pipe, and if he calculates the maximum resistance the engine should overcome to be equal to a head of 200 feet, he puts up a stand-pipe 200 feet high, and from the top of the stand-pipe he brings a return pipe which goes to the main. Between eight and eleven o'clock in the morning, when so large an amount of water is taken in London from the mains, there is a very great draught upon them, and it is impossible to maintain 200 feet in them, in fact, there is not maintained above 150 or 140 feet close to the engine-house; so that for the purpose of using the Cornish engine you pump water up to the maximum height of 200 feet, and let it immediately tumble down on the outside uselessly 40, 50, or 60 feet. Thus it is that although such an engine may appear to be working economically when the consumption of coal is compared with the stand-pipe lift of 200 feet, it may nevertheless be working very wastefully when that consumption is

judged of by the 140 or 150 feet of resistance of head actually required; and moreover there is no necessity for the purposes of economy to have one of these machines, which, in addition to the difficulties I have mentioned as attending its application to the supplying of distributing mains, has the defect of being unlimited in its range, that is of having no determinate stroke. With a double-acting engine working upon a crank the length of the stroke is determined, and leading into an air vessel or into a single stand-pipe (not a double one, going up and coming down again) such an engine is resisted solely by the load which actually prevails in the mains, and the work that engine is doing is real, whereas a Cornish machine requires a constant maximum pressure, although the mains may not require that constant maximum pressure. I say, therefore, that is an instance of how the engineer is misled by fashion. There are cases where single stand-pipes are used with Cornish engines, and there you will actually see throttle-cocks put on to the foot of the stand-pipe to keep the "head" up, and throttle-valves put in the equilibrium-pipe to prevent these engines from having only their right amount of work to do when that amount of work happens to be too small to balance the working weight.

To revert to the question of expansion; in the Cornish engine the steam is cut off by the shutting of a double beat valve (the valve which Harvey and West have introduced also into their pump work), and we have also in slow-going engines valves lifted by cams and dropping at proper intervals. We have then in quick running engines, the Corliss valve moved by an eccentric and brought back to rest by a pneumatic cylinder (which on account of the risk of an air leak, I cannot help thinking is a dangerous thing, I would rather see a steam cylinder than a pneumatic one). The Corliss valves are liberated from the eccentric rod by a spring catch connected with the governor. But one of the commonest modes of making expansion in our ordinary engines, is to have one slide valve upon the back of the other, each valve worked by its own eccentric. By that contrivance it is possible to cut off at almost any part of the stroke. There is no doubt a certain want of sharpness in the cut off, and you do not get on the indicator that beautiful-looking figure which you do by the Corliss valve, or the drop valve of any kind, but when one comes to work it out,

the loss by the imperfection of the outline of the figure is extremely small. There is also the expansion used in the *locomotive* by the *link motion. I perhaps ought not to dwell* upon that now, for if time should permit I may say a few words about the locomotive engine.

The objection to all expansion in single cylinder engines is the variation in the load and strain throughout the stroke, and the necessity of making the cylinders of great size, and also the proportioning of parts so as to bear the initial strain, although the average pressure is small when the expansion is high. To diminish those difficulties double cylinder engines (the Hornblower or Wolfe engines) were invented, and are now largely employed. Of these there have been several constructions. A common one with beam-engines is to put the high-pressure cylinder nearer to the centre of the beam than is the *position in which the low-pressure one is placed,* and by the shorter stroke thus obtained and by a diminished diameter of bore the desired proportion of contents of the two cylinders is obtained. We have had also cylinders placed side by side at the end of the beam; we have had the high-pressure cylinder placed within the low pressure, and their piston rods coupled up to one cross-head. In this case the internal cylinder should always be steam-jacketted, but I do not look upon this arrangement as a very happy one.

We have also had the cylinders placed end on by putting the high-pressure cylinder below the low-pressure and letting the piston-rod come through. Great difficulties have been experienced in arrangements of this kind by having inaccessible stuffing-boxes, and so on, but these have been cured in later constructions. Then we have had McNaught's system, which possesses great advantages for the conversion of non-expansive old-fashioned beam-engines into expansive engines by the application of a second cylinder. McNaught puts his expansive cylinder at that end of the engine which is between the beam centre and the crank shaft, and thereby instead of taking the strains of the two cylinders through the beam-gudgeons and the bolts, the strain of the high-pressure cylinder is brought on the crank side; in that way an engine which would not stand the application of the high-pressure cylinder put in the ordinary position, will support that application very well. These are the modes in which compound cylinders have been employed. There may

be others which do not occur to me at the moment. Consider ing the principles of the double-cylinder engine it is readily seen that if the steam were simply used throughout the full stroke in the small cylinder and were then turned into the large one, there would not be any reduction of initial strain whatever on the piston of the large cylinder nor on the parts attached thereto, because if there were no loss of pressure, although there would be a constant decrease as the steam expanded, yet, at the moment of turning it on there would be the same pressure on the low-pressure piston as if you turned the steam in directly from the boiler. But now-a-days we expand in the high-pressure cylinder to a greater or less degree, and thus the piston of the low-pressure cylinder does receive much less strain than if the boiler steam were turned upon it. This arrangement tends to diminish the pressure on the low-pressure cylinder, and it tends also to equalize the strain, producing a more equal tangential force, but these advantages are *obtained at a very considerable cost.* I am sorry I have not got a diagram here, which would have shown you practically why it is when you have a compound cylinder engine you obtain no more work out of the two cylinders than you would obtain if you put the same quantity of steam into the low-pressure cylinder alone, and gave it the same amount of expansion. I do not say the high-pressure cylinder is not doing work, because it is, but that which it does is abstracted from the low pressure cylinder, and in that way, therefore, one has all the complication and expense of the two cylinders, and the final result got out is no more than if one had a single cylinder of the same size as the low-pressure cylinder—not as the aggregate size of the two—and expanded the steam in that cylinder to just the same extent. We have thus a very large amount of complication introduced into an engine for apparently a very small end. However, it is so introduced, and it is the ordinary construction now-a-days in marine steam-engines. It is said there is a utility in it, because one not only diminishes the strain on the reciprocating parts, but the tangential forces are equalised to a great extent. That is true, but I want you to see what the value of that is. I have here Diagram 10, which shows by Fig. 1 an engine working without any expansion, and by the figure below it the tangential effect in producing the motion on a crank.

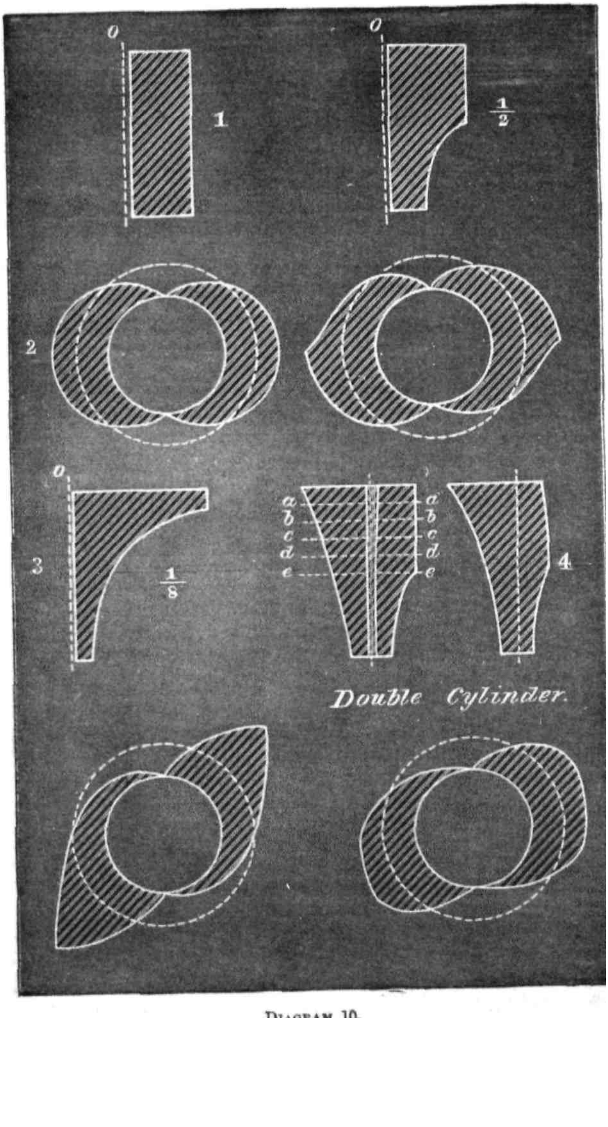


DIAGRAM 10.

Obviously, the non-expansive engine has a tendency to produce variation of motion, that is to say, the tangential force will be zero when the crank is on the centre, and it will be at the maximum when the crank is at half-stroke. In constructing these diagrams I assumed the connecting rods to be of infinite lengths, and we need not therefore take into account the variation that would arise from the effect of the angle made with the connecting rod. By the side of this we see an engine cutting off at half-stroke, and you will see now by Fig. 2 below it that the variation in tangential force is very considerably increased; Fig. 3 shows an engine cutting off at one-eighth stroke, and you will see below the increase in inequality of the tangential force. Fig. 4 shows a diagram from a high and a low-pressure cylinder having the respective cubic capacities of one and four, and wherein the steam is cut off at one-half the stroke in the high-pressure cylinder, so that it is expanded eight times, as in the case of the single cylinder engine considered in Fig. 3.

Fig. 4 shows the aggregate effective pressures of the two pistons at various points of the stroke, and below this the variation in tangential results obtained from these cylinders. There is no doubt whatever that by using two cylinders, we have been able very much to reduce the irregularity in the tangential force which existed with a single cylinder when cutting off at one-eighth stroke. I will also call your attention to another Diagram (11) showing the benefits arising from having more cylinders than one with cranks placed at different angles, so that with two cylinders when one is on the dead point the other is doing its fullest duty. An inspection of this Diagram 11 will show that with a pair of cranks at right angles, and with two single cylinders cutting off at half-stroke, there is a great approximation to uniformity of tangential force throughout the whole revolution.

Let us now consider whether it is worth while to go to the complication of a double cylinder engine for the purpose of obtaining this greater uniformity of tangential force. Nobody objects to the use of a single cylinder non-expansive engine. One is content with that, and no one uses the word irregularity. I should now like to ask you to bear with me for a short time while I go into the question of the fly-wheel. It is true that the fly-wheel cannot give absolute regularity

of rotation; in fact, if the result were absolute regularity the fly-wheel would be an entirely useless implement. Its object is to store up, when the rotation is in excess of the mean speed, a certain amount of energy in order to deliver it out when the rotation is below that mean speed: therefore, for it to be of any use, we must presuppose that a certain amount of variation is to be permitted. What we have to see is how much variation we can do with, and

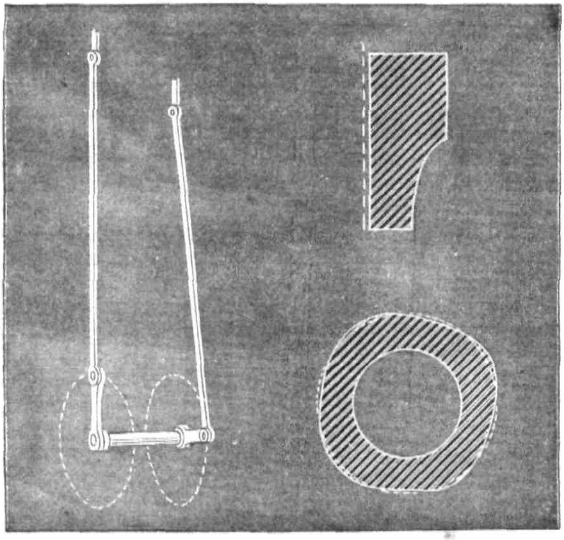


Diagram 11.

then see what kind of fly-wheel we ought to employ. Really a fly-wheel may be likened in its operation to a banker's account. Supposing a man has an income of £1 a day, and he spends £1 a day, his income and expenditure are *equal* and *continuous*, as he spends every day his income as he gets it; such a man does not want any balance at his banker's at all. But suppose he spends £1 a day, and gets his income paid to him once in ten days; if he has not

got a balance of £10 at his banker's he is insolvent for certain periods, and therefore he must have a £10 balance. Supposing he gets his income paid every hundred days, he must have £100 balance. Therefore, similarly as we get more or less irregularity in the tangential force, so must we have a larger or smaller fly-wheel, which we may look upon as analogous to a banker's balance. I will take an instance, and ask you to consider that we have a non-expansive engine developing 240 gross indicated horse-power at 60 revolutions a minute; that will be two horse-power for every half revolution, equal in round numbers to 30 foot tons. Now you will find if you calculate out that tangential diagram for a non-expansive engine, that the variation above and below the mean is about 30 per cent., and 30 per cent. of 30 foot tons is 9 foot tons. This 9 foot tons, therefore, has to be stored up in the fly-wheel during the time when we have the excess of pressure, in order to be given out during the time when we have the deficiency. Now if we have a fly-wheel on this engine of such a size (about 13 feet diameter) that its rim is going at 40 feet a second, and if we allow the weight of the rim to be 9 tons, and assume the whole weight of the wheel to be accumulated there, the value of that wheel will be 225 foot tons in round numbers. Now we want to store up 9 foot tons. It is a 9-ton wheel. We want, therefore, to put in that wheel a velocity equal to what it would derive in falling from an extra foot in height. The velocity of 40 feet a second is that which it would derive in falling from a height of 25 feet, and we want to be able to put into it a velocity which would equal that derived from falling from 26 feet, which would be a velocity of 40·8 feet per second; ·8 is one-fiftieth of forty, and therefore under the circumstances of which I have spoken, we have a variation above and below the mean speed of the engine of one-fiftieth of that mean speed, a variation not inconsistent with very many purposes to which engines are applied. If we were to use an 18-ton wheel going at the same circumferential velocity, the variation would be half of this fiftieth, and if we were to use a 9-ton wheel of double diameter we should get only one-fourth of the variation, but if we were to use a fly-wheel of double weight and double diameter we should get only one-eighth of the fiftieth or one four-hundredth variation.

Referring to the diagram of the expansive engine, cutting

off at one-eighth of the stroke, you will find that the variation of its tangential force is as much as 45 per cent. in excess of the variation of the tangential force of the engine without expansion. Now that excess of irregularity may be absolutely neutralized by increasing the velocity of the supposed engine from 60 to 69 revolutions. That is to say, supposing the designer of the expansive engine wanted 240 gross horse-power, he might either obtain it and a sufficient approximation to regularity by an engine running at 60 revolutions without expansion, or he might so proportion the gearing that it should run 69 revolutions. Were he so to proportion it, it is obvious that the mean work done by the piston at each revolution would be $\frac{60}{69}$ths of what he would get in the first case, and taking this reduction of the mean work and the increased effect of the fly-wheel, running at an extra 9 revolutions, you will find that the same size and weight of fly-wheel will cause this highly expansive engine to work with the same approximation to regularity as that with which the non-expansive engine running at 60 revolutions would work. I think this fact is not sufficiently borne in mind when engines are designed for driving any particular kind of machinery.

There is no doubt that the double cylinder engine is the engine of the present day. I do not think its economy really lies in its principle, but that its economy in practice arises from another thing altogether, and that is this, that by making a double cylinder engine you put it out of the power of an ignorant engine-driver to do away with that which you want —high expansion; he must get high expansion; he is compelled to use it whether he likes it or not, whereas with the single cylinder expansive engine he has the power to follow the dictates of his own ignorance, and as a matter of observation I have hardly ever seen such an engine left to the control of an engine-driver but it invariably worked at the lowest possible grade, and, as I have said, I believe that this withdrawal of control is to a large extent the secret of the success of the compound cylinder engine.

Further with regard to expansion, I wish now to speak of the question of the steam jacket. The steam jacket we owe to Watt, and it was condemned by one of our earliest writers on the steam-engine, Tredgold, who said it was merely adding to the surface to be cooled by the air, but we know now that

we cannot work with the expansive engines to any good purpose without a steam jacket, and I will endeavour to show you how that is. I have here (Diagram 12) a section of a cylinder with a steam jacket round it, and this diagram will, I trust, explain why the steam jacket is beneficial. Assume the cylinder to be without a steam jacket, and that then you

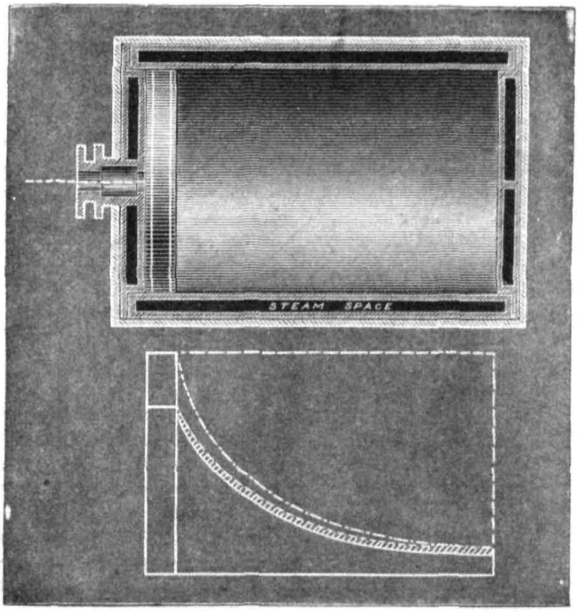


DIAGRAM 12.

have high pressure and proportionately hot steam let into that cylinder, the immediate effect is to condense a portion of the steam and to make it into water. It disappears as steam, and the indicator diagram will only inform us, we had received the steam up to a certain point, whereas in truth another portion has been received but, having been condensed, does not appear on the diagram. As the steam expands and

becomes lower in temperature, the heat which has been absorbed by the walls of the cylinder is given out, and the dew which has been deposited upon them evaporates, and we thus get a final pressure when there is no jacket, very much in excess of that which ought to arise from the quantity of steam that appears to have been put into the cylinder. You will always find in a high expansive non-jacketed engine that the final pressure is very much in excess of that which apparently ought to be there, but if we take into account the steam which has been condensed, you will readily see where this excess of final pressure comes from. Now, had not this portion been condensed, but remained as steam, instead of obtaining only the useful effect represented by the full white line on that diagram, we should have the useful effect represented by the middle dotted white line, which at its termination would coincide with the full white line, but in any other position is outside of it. This difference shows the excess of useful effect obtained by the aid of the steam jacket. You also get the steam superheated by the steam in the jacket during the progress of the piston.

I have spoken of the variations of tangential force, but those are variations which occur while the resistance to the engine is constant; we know that in factory engines, however, the resistance varies very considerably, and therefore one is compelled to have some kind of governor. I wish time admitted of my going into the question, because we have here an interesting series of governors. In the Watt governor the number of revolutions at which the balls will fly out depends upon the vertical height between the point where the arms cross each other in the centre of the spindle and the plane where the revolutions of the balls may be; when the load upon the engine is light, in order that the throttle valve may be sufficiently nearly closed, the balls require to be right up, and to maintain them there, the engine must run at a pace in excess of that at which it runs when it is heavily loaded.

From this cause the Watt governor, where the arms vibrate on a pin in the axis of the vertical spindle, is not such a true controller of the engine as to keep it at regular speeds. The model by Mr. Jeremiah Head I have here illustrates this very well. Here is another very common form of governor which is extremely bad, where the pivot of

the arm is on one side of the spindle; if we imagine the arms produced until they intersect the axis of the spindle, we shall see that when the balls go up, the point of intersection of the arms with the spindle comes down, and in that way the variation in the vertical height of the governor is much greater than when the joint of the arms is made in the line of the spindle itself as in the Watt governor. But Mr. Head improves upon the Watt governor; he takes the arms through the spindle and pivots them on its opposite side; so that when the balls rise, the point of intersection of the arms with the spindle rises also, and that governor, therefore, will act with comparatively little variation in its effective vertical height.

Then we have Dr. Siemen's chronometric governor, which acts as an absolute regulator, and we have also Dr. Siemen's liquid governor, which also operates with absolute regularity. I am only sorry it is impossible to enter into a description of them.

Passing from this subject;—there are two things which in these days everybody knows all about; one is the wire-drawing of steam, and the other is the freeboard of a ship. I am not going to say anything about the latter, but in regard to the wire-drawing of steam there is among the "everybodies" the strongest possible notion that it is a thing to be deprecated. But far from it, in the ordinary construction of bad engines, of which we have so many, wire-drawing is really a thing to be admired. Consider the case of an engine without any expansion valve, a cylinder far too large for its work, with a governor having a throttle valve very nearly closed, and with the steam going into the jacket of the slide valve, and accumulating there until the valve opens to let it into the cylinder. During the time the crank is near its centres, and the piston is going slowly, then notwithstanding the orifice for the inlet of the steam is restricted, an accumulation of steam in the slide jacket is obtained so that at that time the pressure there approximates to that in the boiler; when the slide valve opens that pressure comes upon the piston, and thus we get a sort of irregular, very bad expansion diagram as the indicator figure; but for what you do get it is of value. On this point I must call your attention to Diagram 13, in illustration of what may result from wire-drawing in an engine demanding a mean pressure of 27 lbs.

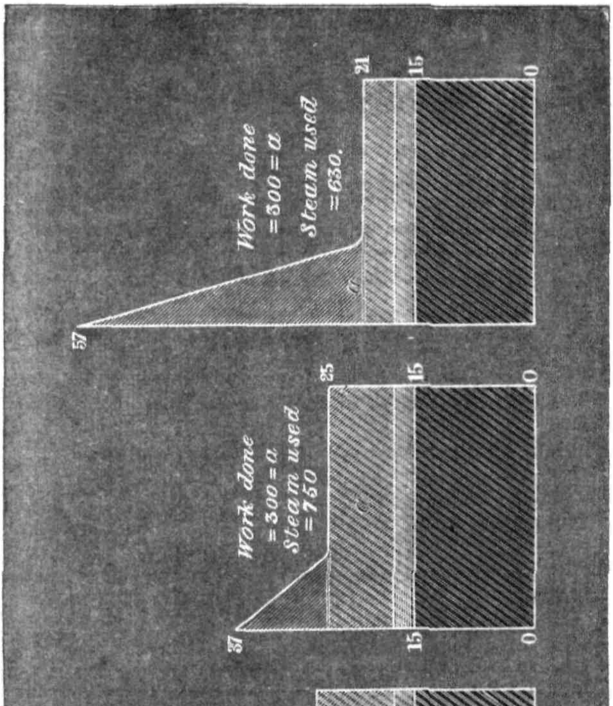

Work done
=300=a
Steam used
=810
27
15
0
a
Work done
=300=a
Steam used
=750
37
25
15
0
a
Work done
=300=a
Steam used
=630.
57
21
15
0
a

DIAGRAM 13.

above atmosphere, without any expansion or condensation. The quantity of steam employed, if there were no wire-drawing, would be expressed by the area of Fig. 1; but suppose now we wire-draw, and put a pressure of 37 lbs. on the piston at the beginning, then the line of pressure may be represented by Fig. 2, and we have a reduced consumption of steam as there shown. Supposing now we wire-draw still further, and put a pressure of 57 lbs. on the piston at the beginning, we then reduce the consumption of steam to the point shown on the third figure, and it really, therefore, is a fact that where you have a bad engine you will do good by wire-drawing. Do not understand me that it is a thing to be used in lieu of proper expansion, but merely as saying that so far from being, as is popularly supposed, a detriment, in every instance, it is, in the case of a bad steam-engine, a positive advantage. Again, take an ordinary engine without expansion, and look at the enormous advantage which will result from putting even a slight lap on the valve. Where the slide travels about four times the amount of the steam opening, you can, with a single slide, cut off at three-fourths of the stroke, and by that arrangement you clearly introduce into the cylinder only three-fourths of the steam, while you have not diminished the work by even $\frac{1}{32}$. Therefore, in every bad engine, it is very easy for the first mechanical engineer in the neighbourhood to do his part towards effecting some saving in our store of coal.

Engines should be constantly examined by the aid of the steam-engine indicator. The original indicator as devised by Watt is shown by the diagram on the wall, with the reciprocating card travelling at right angles to the path of the pencil caused to move by the pressure of the steam. This indicator has been largely improved by Richards, as shown in the further diagram. The improvement has consisted in diminishing the piston stroke, and multiplying it by means of levers, so that the momentum of the indicator is greatly reduced. In that way, and without any alteration of the indicator as ordinarily sold, it is possible to take thoroughly good diagrams of engines running up to 300 revolutions a minute. It fell to my lot, however, to have to take a diagram from one of Thorneycroft's quick steam launches running at 600 revolutions a minute, and I was very desirous, for the purpose of a paper I was writing, of obtaining the vary-

ing power of these engines at from 200 up to 600 revolutions. To do that it became necessary to take indicator diagrams, and the way I set about it was by limiting the stroke of the indicator, and by employing an extremely strong spring. I got for 120 lbs. steam only one inch rise on the figure. I then limited the circumferential traverse of the indicator in the same way; I provided it with an india-rubber spring, so as to make it speak quickly, and in that manner, by having a diagram not more than an inch in either direction, I obtained most perfect and accurate figures from an engine running at 600 revolutions. You will see them published in the *Transactions* of the Institute of Naval Architects. By the indicator we are able to tell, not merely the total work that the engine is doing, but whether it is doing it economically and efficiently or not, and if not, are enabled to say where an alteration ought to be made. The indicator gives us the gross indicated horse-power, from which has to be deducted the friction of the different parts of the engine. I may say upon that, that at the Royal Agricultural Society, when experimenting we had always used the dynamometrical brake, the actual brake which is shown in this Exhibition. On the last occasion, still using this brake, we put on indicators, and were thereby enabled to obtain the ratio of effect on the brake as compared with the indicated horse-power. Speaking from recollection it averaged, taking one engine with another, a difference of 17 per cent. These were non-condensing engines, but I should call your attention to the fact that they were worked in the excellent manner I spoke of yesterday, and yielded results so high, that in one case we got 79·49 million pounds raised one foot for 1 cwt. of coal; a duty that probably is not attained by any single engine in Cornwall, for I am sorry to say that the Cornish engines have very much fallen off in their performances.

I should like now to say a few words about the injector. We have here, cut open, one of the Giffard injectors so commonly used for feeding boilers; and I remember distinctly when this instrument was first brought to my attention by Mr. Robinson, of Sharp, Stewart, & Co., I and several engineers were together, and, on being told by Mr. Robinson that there was an implement by which the steam from a boiler could be caused to generate a jet of water so powerful as to enter that same boiler against the pressure within it, with

the arrogance, I am sorry to say, common to many of us, I said, "nonsense," and we all said "nonsense," and there was a chorus of "nonsense," and nonsense it was, for we had to go and see it at work, and there was no doubt about the result. Then came the question why it worked. Why did we see the steam re-enter the boiler from which it had started? There have been a great many papers written upon this subject in a mathematical form, but I have said on more than one occasion, that I arrogate to myself, rightly or wrongly, the credit of being the first to give a popular explanation of the action of the injector. I believe the whole theory is to be explained by one word, and that is "concentration." Let me illustrate what I mean. Supposing there were a cistern containing water with a nozzle upon the side of it near the bottom, from which the water could issue horizontally in a jet. Assume the jet were opposite to an orifice in an empty cistern parallel with the first. We know that the jet of water would enter that orifice, that the water would accumulate in the cistern which the jet entered, and that the jet would continue to enter until the accumulated water in the second cistern caused the jet to have such an opposition that it could not enter. Now Mr. Froude has shown us in his experiments that when you have once got the water into motion, if you will allow a sufficient difference of head between the two cisterns to compensate for the resistance caused by the friction in the tubes through which the water passes, the water will, if you have a proper nozzle, continue to enter into the second cistern when it is full up to the height of the first one, minus the difference expressing the friction. We see, therefore, that we may get the water entering the second vessel very nearly to the height from which it issued from the first. But supposing that the water in its passage across the imaginary space from the vessel from which it flowed, to the one into which it is to enter, could by some process of magic be converted into mercury, the same weight of mercury as was equal to the weight of water: we know that if we could do that we should diminish the area of the stream to $\frac{1}{14}$ or thereabouts, so that the stream which came out of an orifice of a square inch, when converted into mercury would enter the other tank through an orifice of $\frac{1}{14}$ of a square inch; but the stream being of the same weight and flowing with the same

velocity, would obviously concentrate on that $\frac{1}{14}$ square inch the whole of the energy which, if it remained water, would otherwise have been applied on the square inch, and thus by this act of concentration there would be 14 times the power per unit of surface, and it would come with the same effect as if it came from 14 times the height. With such a concentration it would enter into the second tank not merely against a head equal to that from which it came, but against a head very nearly 14 times as great; and you see, therefore, that if you could by magic convert water into mercury, you would then get the power to enter against a very excessive pressure—to re-enter its own tank, and much more. Now, instead of taking water issuing from a tank, let us take steam issuing from a boiler, and that steam without magic we can concentrate, because we can condense it, and having condensed it, we do not concentrate it only 14 times, but 1,696, or 388, or 144 times, or whatever may be the ratio of the particular steam to water. By that concentration, therefore, we have an enormous power, competent not only by itself to re-enter the boiler from which the steam came, but a power so much in excess of this as to be able to take along with it the water employed in producing the concentration, that is the condensation of the steam. In that way the Giffard injector does introduce into the boiler the steam that came out of it, and the water that condensed that steam.

Except in one solitary instance (that of the surface of boilers) I have abstained from giving any proportions; still less have I had opportunity to indicate the relations between dimensions and horse-power, and I may say that had I had time to dwell upon this branch of the subject of the steam-engine I should have been met with the curious difficulty of not being able to say what in manufacture is meant by a horse-power. But I may tell you now, that, among the five kinds of horse-power which are recognised (for there are five), I, whenever I have used the term horse-power, have referred to the engineer's horse-power of 33,000 lbs. (roundly, 15 tons) raised through one foot in a minute of time.

Dr. Joule and others have shown us that 772 foot lbs. represent the equivalent of one heat unit, and we know, therefore, that 33,000 foot lbs. per minute ought to be got for an hourly expenditure of 2,565 heat units. Llangennech coal is capable of giving forth 14,718 heat units per lb., and

we ought therefore by an expenditure of ·17 lbs. of that coal *to obtain one horse-power for an hour*, while the very lowest consumption that I know of is $1\frac{1}{2}$ lb., therefore the ratio between those two figures is 9 to 1, so that at the present time the best engines use but one-ninth of all the heat which is theoretically given forth by the coal. We see, therefore, there is before mechanical engineers a very large field for improvement. Do not imagine that I am suggesting that we shall ever obtain the whole of the theoretical effect or anything bordering upon it. Certainly not by the steam-engine, *for we do not yet know of materials which would admit of our* commencing with a pressure where the steam would be of a density equal to that of water, nor could we tolerate cylinders of such size as to enable the expansion to be carried on until the temperature of ice was attained ; and yet those two conditions, I believe, are necessary before we can even theoretically get as a useful effect the whole heat of the fuel, but the efforts of mechanical engineers and of men of science for the improvement of the steam-engine have not been barren of results in the past, and will not, I trust and believe, be fruitless in the future. We are led to be hopeful when we recol lect that within the last fifteen years the consumption in ocean steamers has been brought down from 5 lbs. of coal per gross indicated horse-power, to $2\frac{1}{2}$, 2, and even $1\frac{3}{4}$ lbs. in real regular ocean steaming; and we have in the case of her Majesty's ship *Briton* when steaming at half-power, seen the consumption as low as $1\frac{1}{2}$ lbs. of coal per gross indicated horse-power per hour, which is, as I have said, the lowest that has come under my notice in a real working engine. Is it too much to hope that greater economy will yet be reached ?

I must now bring these two lectures to a close, and in order that they may lead to some practical result I will at this, their very conclusion, ask you, bearing in mind the progress which has already been made, to look at the pair of indicator diagrams to which I now point. These show the immense difference in consumption of steam (and therefore of coal) between two engines, the one (of no progress) working non-expansively and non-condensing, and the other (the engine of progress) working expansively and condensing. Too frequently, from a desire both in the engine-maker and in the engine-user to save a few pounds in the first cost, wretched machines such as the non-expansive non-con-

densing engine from which this diagram was taken, are scattered over the country and are wasting the fuel which it is our duty to preserve for those who will come after us. If you will bear these facts in mind and consider the loss indicated by the diagram of the "engine of no progress," as I have termed it, I shall feel that my labours here have produced some practical results.

I cannot hope that within the limits of these two lectures it has been in my power to bring before you new matter, nor, indeed, is it probable that anything I could say would be absolutely new to an audience such as that which I have just been addressing; the utmost I can hope, therefore, is that these lectures may have been suggestive, and that by bringing to your minds certain points in the construction and management of the steam-engine, and by impressing you with the fact that upon the attention paid to such points economical results depend, I may be the means of inciting each one of you when returned to his own district to exert himself to improve the steam-engine standard in that district, and thus to get rid of the wasteful machines to which I have just alluded; and let me remind you that by so doing each of you may perform his part of the duty that devolves upon scientific mechanical men, viz., that of preventing the scandalous waste of fuel that now, alas, too frequently occurs.

I must apologize to you for the audacity exhibited in my endeavouring to lecture on a subject so extensive as that of the "steam-engine" in a course of only two lectures, but let me plead in extenuation that the audacity is not mine but that of the Committee of Council on Education.

In conclusion let me thank you for the forbearance you have shown, and also for the very great attention you have paid to me.

RADIATION.

BY PROFESSOR G. FORBES.

Among the different physical researches which are illustrated by the collection of instruments at present exhibited, there is one which has performed a very important part in forwarding the progress of a special branch of physics.

This department, which is shown pretty fully by the apparatus on the table, is the department of radiant heat. Yesterday we were discussing the question of the undulatory theory to a certain extent, and we saw that by the measurement of the velocity of light it was absolutely proved that Newton's corpuscular theory of light was untenable, and that therefore, as far as that theory was opposed to the undulatory theory, the undulatory theory must be accepted as true. There are a vast number of other facts, however, which tend to prove the truth of the undulatory theory, both directly and indirectly. Yet there is no proof so absolutely certain of this theory as others, of the theory of gravitation for example. In fact, in physics there are wonderfully few theories which are absolutely demonstrated with the same certainty as the law of gravitation. The law of gravitation as stated by Newton is simply a statement of the facts which have been observed, assuming the definition of force as he gave it. It is not a theory—it is simply a statement of observation, a gathering together of a host of phenomena into one general statement. Just as the whole of the propositions included in spherical astronomy and spherical trigonometry are dependent on the simple statement that a sphere is a surface, every part of which is equally distant

from the centre; so also all the phenomena observed and grouped by Newton are included in the statement of the law of force as he defined force to be.

But there is no other theory in physics so firmly demonstrated as this. Our belief in the truth of the undulatory theory rests on the fact that it explains a vast number of totally different phenomena in a perfectly clear way; in fact, there are no phenomena in the theory of light which, if they have not been explained by the undulatory theory, we are not justified in saying could be explained by it if our mathematical analysis were sufficiently powerful to translate the meaning of the theory completely. But the undulatory theory of light has acquired certainty from a vast number of independent sources as well. In order that the radiations from a luminous body may be propagated through the ether, it is necessary that the luminous body must have its molecules in a state of rapid vibration, and not only has the whole science of spectrum analysis led up to this, to point out that the molecules of different bodies are vibrating in certain definite periods, such as will give vibrations off to the ether, and ultimately send them so as to strike the eye, but also a number of theories which have been reached independently, all tend and converge to this same point, and show us that not only are the molecules of a body in a state of rapid vibration, but that there is an ether capable of transmitting those vibrations. The splendid experimental results obtained by Professor Graham in connection with the diffusion of gases have proved almost beyond a doubt that the molecules of gases are moved about with a very great velocity, and means have been devised actually to measure the very velocity with which they are moving. The researches of Clausius, Clerk Maxwell, Thomson, Rankine, and others, have reduced the science of the molecular motion of gases to an absolute certainty, so far as theory can be certain, and they have explained the laws of gases perfectly upon this assumption. The very size of one of these molecules has been determined by Sir William Thomson in four different manners, all of which agree, within fair limits, with each other; and from arguments which it is impossible for me to go into at present, Sir William Thomson has shown what is in all pro-

bability the actual nature, form, and motion of those molecules. *He shows that an atom is in all probability* nothing more nor less than a vortex ring. You have all seen smoke rings which have been formed from the mouth by tobacco smoke, or which are often seen coming from the mouth of a cannon—a ring of smoke passing through the air with considerable velocity, and this ring having convolutions from the exterior of the ring into the interior round and round. Such a smoke ring is the fundamental idea of a vortex atom as defined by Sir William Thomson. These results may seem to be too hypothetical, but on a clear examination of the grounds which have led him to adopt these views, there is very little doubt that if not the true explanation of the nature of a molecule it is something *very close to it.*

Furthermore as to the existence of the ether, we have several independent proofs. The illustrious astronomer Encke found that the comet which bears his name was retarded year by year.

Each time this comet appeared in its elliptic orbit round the sun, it was found to have a shorter period, and to be revolving quicker round the sun. Now we know from the law of gravity, that if it be revolving quicker round the sun, it must be closer to the sun, and the only force which Encke could conceive of to bring the comet close to the sun was the resistance of an ether to its motion which increases the force which the sun exercises upon it in *relation to its momentum forwards.* Various astronomers have calculated the effects of this, and the general impression is that there is a resisting medium acting upon this comet, but that the exact law of resistance which was assumed by Encke is perhaps not exactly correct.

But we have furthermore some most remarkable experiments by Professors Tait and Balfour Stewart, which appear to point most clearly to the existence of an ether. Here we have the remarkable apparatus which was employed by them. It consists of a receiver which can be exhausted by means of a powerful air-pump. Within this receiver there is a disc which is capable of rapid rotation, and mechanism is applied to it by means of which this rotation can be given. A rod acts upon this mechanism and passes down through a barometer tube, so that it can be easily turned

by hand or by other mechanism, and force can be applied to the disc without affecting the vacuum at all. There is also an instrument which I will describe presently, called the thermopile, and by an ingenious arrangement of a barometer tube a motion can be given to the thermopile without affecting the vacuum These gentlemen experimenting with this apparatus exhausted the receiver of air as completely as it was possible to do, and then put the disc into very rapid rotation for a considerable time, and testing it with a thermopile, it was found to be heated. I cannot go into the precautions which they took to make sure that this heat was not due to friction or to the resistance of the air, and so forth; but they took careful account of these things, measured the amount of these sources of heat as far as possible, and allowed for it, and still there was outstanding an amount of heat which could not be accounted for except on the assumption that it was produced by the friction of the ether.

Now the series of researches which we illustrate to-day is one which connects the undulatory theory of light with radiations generally, and with heat radiations in particular. The first experiments which proved that there was some analogy between heat and light were probably made by Porta in the sixteenth century. He employed mirrors to reflect heat, arranged as you see them here. He was able to converge the heat to a focus, just as you can converge light to a focus, and this experiment was repeated by Pictet at the beginning of the present century. Pictet employed the very mirrors which you see before you. He so arranged them that the light placed in the focus of one mirror was seen to throw an image of itself by the double reflection from these two mirrors upon a certain definite point. Having found that these were in the luminous focus, all that remained was to show that they were in the heat focus also; and in order to do this Pictet employed a freezing mixture, and showed not only that the heat was reflected, but apparently that the cold was reflected too. If we place a freezing mixture in the focus of one mirror we shall probably be able to see that the air thermometer in the focus of the other shows a sensible production of cold. But that does not mean really that cold is radiated from the freezing mixture, but that heat is being radiated from the

bulb. Heat is radiated from every part of the room, from the freezing mixture as well as from the walls, but there is less heat radiated from the freezing mixture than from other parts, whereas the bulb of the thermometer will be radiating heat not only to the walls of the room, but also to the mirror and thence to the freezing mixture. There is a continual exchange of heat going on between the thermometer and surrounding bodies,—if they are all at the same temperature the thermometer does not change, because it

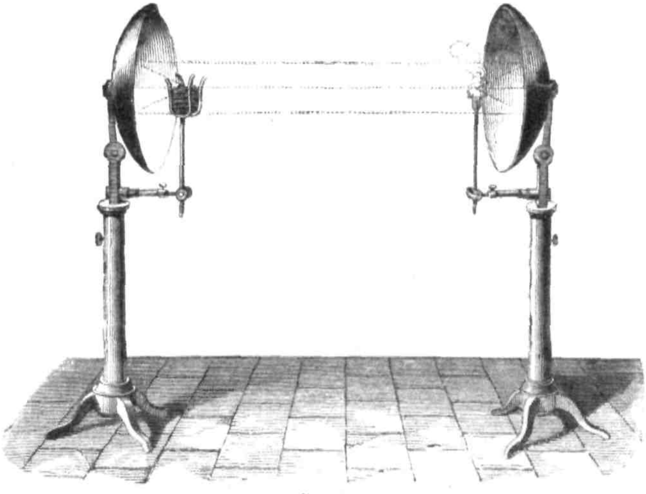


FIG. 1.

is radiating as much heat as it is absorbing; but if we place a freezing mixture in that focus, the thermometer will radiate to that freezing mixture a certain amount of heat greater than the amount of heat it gets from it.

After this was stated by Pictet in Geneva, there being a large band of philosophers bound together at Geneva, they continued to take up this, and to examine the laws of radiant heat with some care. The most important results that were obtained were those obtained by M. Prevost. He published a theory of the law of exchanges which has

been amplified by others and which was brought before the world in the writings of Professor Balfour Stewart.

The theory of exchanges is simply that which I was speaking of just now—that we are not to consider when the thermometer, say, is heated by radiation, that it is simply receiving radiations from a hot body, but also that it is giving off radiations at the same time,—that it is radiating its heat to surrounding bodies. When the thermometer does not move, we know that there is an equilibrium of temperature and that consequently the quantity of heat it is radiating is equal to that which it is receiving. If it rises we know that it is receiving a greater number of radiations than it is giving off; and on the contrary, when it is falling it is radiating more than it is absorbing. The theory which Prevost enunciated, called the theory of exchanges, leads us directly to several very important results which were proved to follow from this theory partly by Prevost and partly by Fourier. These facts are stated in the following table:

1st. All surfaces absorb as much heat as they radiate at the same temperature.

2nd. The quantity of heat radiated increases with temperature.

3rd. The heat radiated varies inversely with the square of the distance.

4th. It also varies as the cosine of the angle of radiation.

The most important of these propositions is the third which was proved by Fourier; but we will take them in order. I have already pointed out the reason of the first one; because if this bulb did not radiate the same quantity as heat as it absorbs at its present temperature—suppose it radiated more heat than it absorbed—then although all objects around it are at the same temperature, still we should see the thermometer gradually falling; whereas if it radiates less heat than it absorbs, the bodies all round it being at the same temperature, we should find that the thermometer would indicate an increase of temperature and it would be gradually rising. The second law, that the quantity of heat radiated increases with the temperature is evident, because the surrounding bodies are hotter than this thermometer. The thermometer increases in temperature, therefore the thermometer is absorbing more heat than it is radiating; therefore the

surrounding bodies are radiating more heat to it than they would radiate to it if they were at the same temperature as the thermometer itself. Therefore we know that these surrounding bodies, when they are at a greater temperature are radiating more heat than they would if they were at a lower temperature. The third law was shown by Fourier also to follow from the theory of exchanges at equilibrium of temperature. He supposed a large heated surface to shine upon a thermometer or upon a thermopile. Suppose that wall were heated, this instrument receives, when close to it, a certain amount of intense heat from a small surface of the wall. When I double the distance the surface of the wall which is radiating into the thermopile is four times as great, and when I treble the distance, is nine times as great. The surface of the wall radiating heat into the thermopile will always be proportionate to the square of the distance ; but upon making the measurements experimentally we find that the temperature registered by the thermopile is the same in all cases. In consequence of this, since the surface which is radiating heat increases with the square of the distance, we know that the intensity of radiation from any portion of that surface must diminish in proportion to the square of the distance. The fourth law is also proved in the same manner, because if I keep the thermopile constantly at the same distance and incline it to the wall giving off the heat, we always find the temperature registered the same. Now when it is perpendicular to the wall there is a small surface of the wall giving off heat; but when I incline the thermopile at an angle there is an elongated space in the wall giving off heat, and yet the temperature is the same. The surface is diminished in this case in proportion to the cosine of the angle between these two directions, and since the temperature is the same, although the surface is lessened in the ratio of the cosine of the angle, it follows that the intensity of radiation from any portion of the surface is proportional to the cosine of that angle from the perpendicular.

Some of the most important experimental results in this subject were those obtained by Professor Leslie, of Edinburgh. Unfortunately we have none of the actual apparatus which he employed; but the instrument which he made use of was a curve of this sort, which was called a differ-

ential air-thermometer. There is a bent tube with liquid in the centre of it terminating in two bulbs. If one of those bulbs is heated the air in it expands considerably, and this will be shown by a rising of the liquid in one arm of the tube and a sinking in the other arm, whereas if it were cooled the contrary would take place and the liquid would actually descend in this arm and rise in that arm. This instrument, together with an apparatus called a Leslie's cube, which consists of a cube of metal with sur-

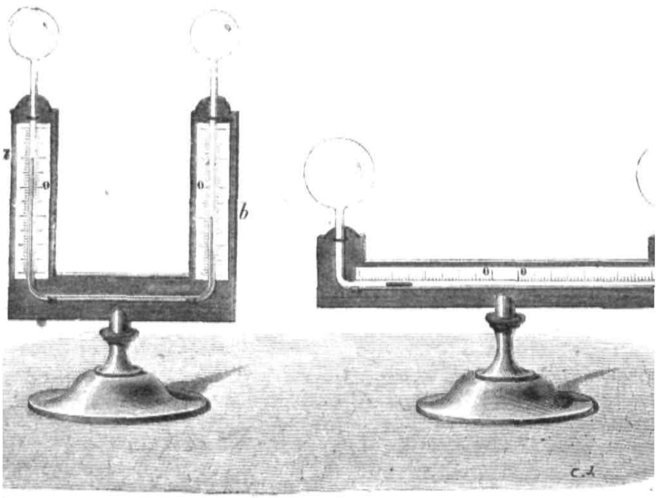


FIG. 2.

faces of different natures, some highly polished, others blackened or whitened, and so forth, were the chief things employed by Leslie. He arrives at the result that different surfaces radiate heat to different extents. Thus if he employed a polished surface and heated the cube with boiling water, the thermometer was much less heated by radiation than if he employed either a black surface or a white surface; in fact, he drew out a table of a large number of different substances, and showed the relative different

radiations from these substances when a source of heat which was not luminous was employed.

These researches led Dr. Wells, in the year 1815, to his theory of dew. Dr. Wells observed that if radiations are taking place from all surfaces of the earth—from the surface of grass, metals, and other objects—then when the sun is away so that the atmosphere is not heated, those objects ought all to get cool by radiation, throwing heat off into space. But following out the conclusions of Leslie, he said there are some substances which ought to radiate heat very much more than other substances. For instance, grass and the leaves of trees, or the substance of which a spider's web is composed, are all good radiators, and those ought to get cool quicker than a metal or any substance like that which does not radiate so much. He also saw that if any object be covered at a little height, however thin that covering be, it will prevent free radiation from the surface of the ground to the sky, and consequently it will not cool so rapidly. He noticed that those were all the conditions which were favourable to the deposition of dew, and he saw in this an explanation of the formation of dew. The atmosphere always contains a certain amount of moisture; if it were to be suddenly cooled the amount of moisture which it would be capable of containing in the form of vapour would be sensibly diminished. If now in the atmosphere there be a sufficient amount of moisture so as to saturate it, and suppose that to be cooled, it will no longer be able to contain this moisture in the form of vapour, and consequently it will be deposited in the form of dew on any surface which has cooled down to below the dew-point of the air at that time. I shall be able to make this clearer perhaps with the aid of a very rough experiment.

You know that if one breathes upon a piece of glass it will be covered with vapour deposited in the form of minute drops of water, the reason being that the breath is saturated with aqueous vapour. The glass when the breath falls upon it is cold; at that temperature the air can no longer suspend so much moisture in the form of vapour, and consequently it must be condensed. This, then, illustrates the manner in which dew is formed on surfaces

which radiate most heat; but if, previously to breathing upon the glass, we slowly heat it so as to raise it to the temperature of the body, then when we breathe upon it we do not cool the breath below the temperature of saturation, and we shall find that it can no longer condense the vapour which is contained in the breath. The glass is absolutely clear and transparent after being breathed upon.

Simultaneously with these advances in the science of radiant heat, some very curious researches were being made which tended to divide heat into different kinds. Sir William Herschel had observed that the temperature of the spectrum varies in different parts. He formed a spectrum with a prism of glass, and, passing a thermometer along the different parts of the spectrum, he found the temperature increase as he approached the red end, and not only so, but he got a very large amount of heat in the part which was utterly invisible beyond the red. This was a very important advance, and was taken up afterwards by other celebrated experimenters—by Seebeck, the illustrious discoverer of thermo-electricity, who experimented on light and other subjects. He found that the position of the maximum point depended on the nature of the prism which was employed; thus when he employed a prism of flint glass he found the maximum was beyond the red, but when he employed one of water he found that the maximum was in the yellow. The next person who threw light on this subject was Delaroche. He employed a luminous source of heat, and measured the effect which it had upon the thermometer, but he found that as soon as he placed a piece of glass between the luminous source of heat and the thermometer, the thermometer indicated an increase of temperature very much less; that there was only three or four per cent. of heat transmitted through the glass; but when he employed heat which had already passed through one piece of glass, then on putting another piece of glass in front of it, he found that the temperature was hardly diminished at all; he found that heat, in fact, which had once passed through a piece of glass could easily pass through another piece of glass also. Thus there seemed to be some reason for dividing heat into two kinds—one

which could pass through glass and the other which could only pass with difficulty: the effect of introducing the first piece of glass was to cut off one kind of heat. This subject was fully investigated in a splendid series of researches by Melloni; he was led to take up these researches by associating himself with Nobili, an Italian, who has the merit of inventing this beautiful instrument called the thermopile, an instrument which renders experiments in radiant

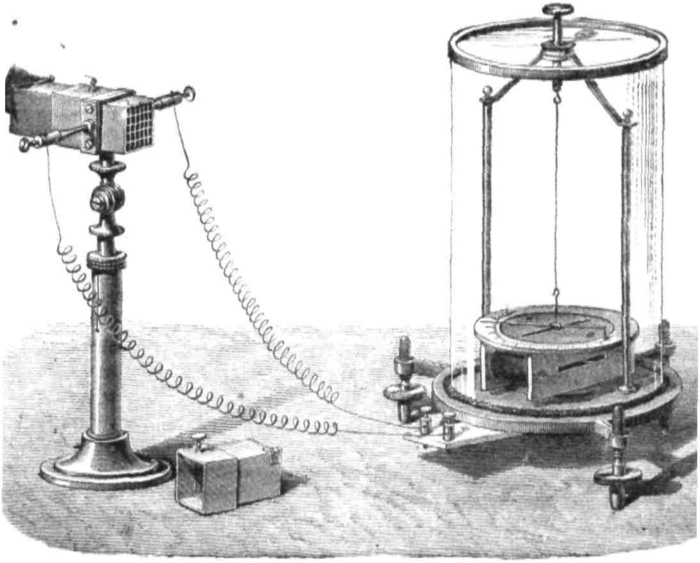


FIG. 3.

heat practicable in many cases where they would not be so without it. I will not waste time in describing the principle of the thermopile, except to say briefly that there are a number of bars of antimony and bismuth joined together *in pairs alternating at the two ends.* It had been found

by Seebeck that if either end were heated more than the other, a current would pass through this continuous series of bars and through a wire which might be connected with the extremity of the series and with a galvanometer, so that all that was necessary in order to measure the increase of temperature in one side over the other was to measure the intensity of the current of electricity passing through the wire. Here is another thermopile, and we shall see presently, as soon as the beam falls on the surface, a *current of electricity* will pass through it and be indicated by the motion of the galvanometer. Employing this instrument, Melloni was led to other important results. The most important perhaps was his grand discovery that rock-salt is a substance which is apparently absolutely permeable to heat. He believed there was no loss of heat by absorption when radiant heat was passing through rock-salt, but it is better to say that there was no absorption perceptible with the instrument that he employed. Unfortunately rock-salt is a substance that deteriorates very much by exposure to a moist atmosphere, and consequently it cannot be employed in laboratories except under peculiar circumstances, and it has to be preserved in a very dry atmosphere. I have here a lens, a prism, and a plate of rock-salt, which are exhibited in the Loan Exhibition by Stegg, of Hamburg; and in order to preserve them they are surrounded by pieces of glass. Almost all the great researches in radiant heat have been made by the aid of rock-salt apparatus. Melloni measured the amount of heat which is found in different parts of the spectrum, and also contained the method which was employed by Delaroche of sifting heat. We find his results included in a considerable-sized volume called *La Thermochrose*, or *Heat Coloration*, we may call it; and he used flames and sources of heat of different kinds. He found different results from different kinds of heat. Thus when he employed a flame sent through rock-salt he found only 92 per cent. got through, some being reflected from the surface. When he employed plate-glass only 39 per cent. got through, and with alum only 9 per cent. Then when he employed other sources of light he got the heat proportionately, as shown in this table:

MELLONI'S EXPERIMENT.

	Flame.	Incandescent Platinum.	Surface at 700° F.	Heat at 212° F.
Rock Salt .	92	92	92	92
Plate Glass .	39	24	6	0
Alum . . .	9	2	0	0

You will observe that with non-luminous heat, alum refuses to allow any heat to pass through it at all, although it is a substance which is perfectly transparent. Plate-glass is a substance which lies half-way between rock-salt and alum, the loss being only about 10 per cent. ; so that he got 90 per cent. of the heat through the second piece of alum, clearly showing that there are certain rays which are absorbed by the alum, and all the other rays are allowed to pass quite freely through it. In fact, he saw that every substance had the property of absorbing certain colours of the spectrum, and that heat differed in no degree from light ; that there was a heat spectrum as well as a light spectrum, but that the great intensity of heat was as a general rule in parts of the spectrum which were invisible ; and the reason of this is simply that the substance of which the eye is composed does not allow these radiations of heat to pass through. The general results of Melloni's work may be represented by a diagram something of this sort in which the upper part shows the curves representing the intensity of radiation at different temperatures with the curve of limiting visibility, whilst in the diagram beneath is given the resulting curve of apparent visibility. The thing wanting to show the absolute identity of light and heat was some experiment on the phenomena of heat analogous to those which had been performed on the theory of light which acquired the name of polarised. You know that if we employ several pieces of this substance called tourmaline, which is tolerably transparent, if we first pass the light of a candle through one piece and then through another, we can turn this piece of tourmaline about in a certain direction until we get total darkness ; that is to say, when the two pieces of tourmaline are

arranged in a certain position no light can pass through them, but if you turn one of them at a right angle then the light can pass freely through both of them, and that is the phenomena called polarisation. It was completely explained by the undulatory theory of light and by that theory alone, and it became desirable to see whether such a thing could be done with heat from a low source of temperature which was not luminous. Various experiments had been made in the early part of the century to test this, but with only negative results; until hearing of the employment of this new form of instrument for measuring temperature, viz. the thermopile, the late Principal Forbes employed the instrument in a re examination of this question. He had already tried the experiment with other kinds of thermometers with negative results, but on employing this thermopile with two pieces of tourmaline he was enabled to show the fact that there was polarisation, and this having been once established seemed to make it at once certain that light and heat were absolutely identical, because the source of heat which he employed was a vessel of boiling water which was non-luminous; but he continued to examine the question further, and at last obtained the means of measuring with very great accuracy the amount of polarisation. Here are two of the instruments which were actually employed in these researches. They consist simply of a cylinder and a piece of mica inclined at a proper angle inside it. You know that if we take a large number of plates of glass and lay them one over the other we can reflect light at a certain angle or transmit it at the same angle, and the portion of light transmitted or reflected is polarised; and the larger the number of plates of glass, the more complete is the polarisation. In order to get an analogous phenomenon in the case of heat, he employed

FIG. 4.

mica because he had observed that mica transmitted a large proportion of non-lumious rays. He devised means of splitting mica into a large number of minute plates by exposing it to the action of heat. By so doing he was able to keep the mica in one mass, and yet to have that split into a large number of plates, and this was the material employed which led to successful observations being made on the polarisation of heat. Before illustrating this I will point out the table of Principal Forbes' results, which is as follows :—

FORBES' EXPERIMENT.

Refractive Index of Rock Salt.

Heat from lime, mean value	1·531
Heat passed through glass	1·547
Heat passed through alum	1·558
Mean luminous ray	1·562

This proves in another way the identity of luminous and non-luminous vibrations. By employing the angle of total reflection with a prism of rock-salt, and measuring this angle, Principal Forbes was able to find by simple geometrical principles the refractive index of rock-salt for different sources of heat. He first used the heat of lime, then, after passing it through glass, he found a different refractive index; and then when he passed it through alum he got a third value, and from the mean luminous ray he got a fourth value. You see the only difference is that the lower the source of heat the more near it is to the red end of the spectrum, or beyond the red end the greater is the refractive index, which is in accordance with theory.

I will now try to show you with this apparatus the experiment on the polarisation of heat. Unfortunately these instruments, which were employed by Principal Forbes, and which have been lent by Professor Tait, are in an extremely fragile condition, and are also covered with dust. I hardly like to run the risk of injuring them by wiping the dust off, and therefore I will use a slightly different form of apparatus, which will answer the same purpose. I have here two large prisms which have been lent by Prof. Guthrie. By means of this large Nicol prism we can get a polarised beam of light. This is called the polariser,

and the next is called the analyser; and when the light is passed through this it is in a peculiar condition—it is said to be polarised; but if after that I turn this round its axis in different directions, I shall find that in some positions it will cut off all the light, but in others it will allow a considerable amount of light to pass. If our adjustments were perfect, it would now have totally disappeared, but there is a difficulty in preventing a slight reflection from the interior of this prism. Now I wish to show the same phenomena with respect to heat; but before doing so I shall show you another experiment which we also have with regard to light. When we have turned this round until we get a total obscuration of light, or very nearly so, I will pass between the polariser and the analyser a piece of crystal mica, and that has the effect of altering the condition of this ray and depolarising it so that it is again at liberty to pass through this second prism. You see the colours there, showing that the light is again passing through, and by rotating the mica I can get different colours. In the case of heat it will be most important to employ those colours which are of a deep red, and then we shall be more likely to get non-luminous rays transmitted. You see first the light is polarised by this prism, then on examining it with the second prism we find that in certain positions it is allowed to pass by transmission, whereas in others it is stopped out entirely; but when it is stopped out we get the light passing through the piece of mica again Principal Forbes employed a non-luminous source of heat, viz., boiling water; but since we are unable to employ the same delicate means which he had, on account of not having these mica plates, we shall have to use a luminous source of heat and cut off the luminous portion by means of a solution of iodine in bisulphate of carbon; that will cut off entirely the luminous rays and leave only the dark ones, because this solution is absolutely opaque and does not allow any luminous rays to pass through it, but it permits the ultra red rays to pass through it with great facility. Previously to placing that in the beam I will place this thermopile as nearly as possible in the focus of the rays. You see a deflection produced in the galvanometer by the luminous rays falling upon it. I will now cut them off, leaving only the dark field. The solution

is now interposed, and the only radiant rays which can pass through are the non-luminous ones. I bring the needle of the galvanometer to rest, and now we have a dark field, allowing quite complete polarisation of the light; but when I turn the Nicol prism round until I see the luminous image upon the near side of the iodine, which will be absorbed, and not allowed to appear on the thermopile, you will see that the needle is gradually deflected. The field, which is a bright field, is also a hot field, and when we turn the crystal round so that we have a dark field, we have also a cold field at the same time. Now we have an intense amount of heat getting through; by turning round the dark field you will see that the needle has returned, and we shall have the cold field back again. When it has returned, I interpose between the two prisms the piece of mica which you saw produce so remarkable an effect in the case of light. This experiment is a little more delicate, but still I hope we shall be enabled to get a sensible amount of heat allowed to pass through just as we saw a very sensible amount of light pass through before. You see the needle has moved through a considerable angle since the mica was placed there. Then we will remove the mica, and you will see it return to zero. These experiments were followed by a number of others; and by varying these experiments with the mica polarisers and rock-salt apparatus Principal Forbes proved nearly all the elementary facts of the undulatory theory of light to be true with respect to heat. Perhaps the most striking was the circular polarisation of light. M. Fresnel, by a most wonderful application of mathematical analysis to physics, viz., by an interpretation which he gave to the remarkable mathematical symbol $\sqrt{-1}$, which had been found in some researches of Sir Wm. Hamilton's and others to have a peculiar significance, and by interpreting the significance of the symbol in a peculiar way, was led by the equation he had deduced to a most remarkable discovery, the circular polarisation of light by what is called Fresnel's rhomb, which consists of a piece of glass of a peculiar shape. It is found that when light, polarised in a certain plane, falls on one of those surfaces, and, after two reflections, is allowed to pass out again, the light is in the peculiar condition which is called circular polarisation, and the physical

meaning of that is this—that supposing a ray of light is passing from me, the whole of the ether which is set in vibration is not vibrating backwards and forwards in any particular direction, as is usually the case, but it is set in a circular mode of vibration. As I have already said, Principal Forbes found that by employing a Fresnel's rhomb, made of rock-salt, at the proper angles, he was able circularly to polarise heat also. This and several other experiments I should have liked to show you, but unfortunately I have none of my apparatus here. For this one I am indebted to the kindness of Professor Guthrie, but the result remains the same, that the application of the undulatory theory to heat has been absolutely proved.

Perhaps the only connecting link that was wanted has been supplied in late years to show that wherever there is light there is also heat. The stars radiate to us a large quantity of light, and if we could show that the stars radiate heat to us, it was said by some that this would be a final and conclusive argument of the identity of heat and light. This was taken up by several astronomers, Mr. Stone at the Royal Observatory at Greenwich, Mr. Huggins, and also Lord Rosse. They all found effects of this kind; and Lord Rosse, by means of the apparatus which you see before you, and by two thermopiles used in a differential manner, was able to compare the heat radiated by the moon and the space round the moon, and at the different phases of the moon, by which he was led to very remarkable results; so that the fact has been abundantly proved that both the stars and the moon radiate a large amount of heat to us.

I wish to point out to you with regard to this diagram that the form of the curves is entirely hypothetical. You must not think that they are at all likely to be exact, because we really know very little about the amount of radiation from different parts of the spectrum; it depends very much on the absorption of the substance we employ. Very likely the maximum ought to be moved closer to the red end, but that is a question which we cannot settle definitely.

I have attempted in a very feeble way to pass through the various researches which led chiefly during the first half of the present century to the final identity of light

and heat, and we now know with certainty that the radiation of heat and the radiation of light are absolutely identical, and that the *only* difference between them is in the absorptive power of the substances composing our eyes —that they only allow a certain amount of rays to pass; there are rays not only beyond the red end but also beyond the violet end of the spectrum. Those were first discovered by Ritter, and this was investigated extensively by Professor Stokes in a manner which I hope you will have the pleasure of hearing from his own mouth in one of his lectures in his celebrated experiments on fluorescence.

Of late years no experiments of great importance have been made adding in any manner to this theory, but the science of radiation has been advancing nevertheless. I will simply allude to two pieces of apparatus here. This is one used by Professor Tyndall in his researches on the radiation of gases, or rather on the absorption of gases; but since radiation and absorption are equal, it follows that where we have radiation we have an equal amount of absorption. He employed a non-luminous source of heat. He closed his tube with pieces of rock-salt, heat was radiated through the tube, and struck on this thermopile. The thermopile was in connection with a galvanometer, by means of which the effect was measured. In the most delicate experiments it is necessary to heat also the other side of the thermopile, so that the mode of experimenting is this: The tube is exhausted of air entirely, and the current is reduced to zero by placing a vessel of hot water at the other side, so that this side of the thermopile is as much warmed as the other side is by the radiation from the tube. A cube filled with boiling water is placed at the other end of the tube: it absorbs a certain amount of radiant heat, and this difference is shown by a diminution in the temperature of this side of the thermopile, which is indicated by the galvanometer. The most interesting result of this was that apparently gases which are of the most complex molecular structure had the greatest effect in absorbing the red end of the spectrum. Unfortunately in employing aqueous vapour there is a tendency to deposit it on the sides, and you see that not only is the heat radiated directly through this tube, but also to the sides, and receives a number of

reflections from the sides; consequently when aqueous vapour was employed it was deposited on the sides of the tube and diminished the radiation. It seems that this is the explanation of the very large diminution in the amount of heat transmitted when aqueous vapour was employed in this tube. The celebrated Dr. Magnus employed a different form of apparatus, and also repeated this experiment, and he came to the conclusion that aqueous vapour is not so enormously absorptive of the ultra red rays as Professor Tyndall found; but in other cases Dr. Magnus's results were in complete accordance with Professor Tyndall's.

In conclusion I would draw attention to the apparatus of Professor Balfour Stewart for determining the radiation of the sun at any one observatory day after day. It consists simply of an iron case which has a motion in azimuth and altitude, into which a thermometer can be screwed. This pointed directly towards the sun, and the sun's heat is concentrated, by means of a lens, upon the bulb in the interior of the iron case. I have not time to explain the principle of it, but in all probability this method will give the most constant and most clear method ever devised for measuring the amount of radiant heat from the sun at different times. This is a problem which has always offered the greatest difficulty, and a great deal of credit is due to Professor Balfour Stewart for this invention, which will probably lead to more accurate measurements than have yet been obtained. The subject of radiant heat is so extensive that it would have been interesting to have devoted several lectures to the subject, but I have attempted to-day to confine myself as far as possible to that part which connects the theory of heat with the theory of light.

MICROSCOPES.

BY H. C. SORBY, F.R.S.

In the course of one hour it is quite impossible to do more than give a very brief outline of the subject of the microscope, for there are a great many departments connected with it, each of which would well deserve to have a special lecture. I might treat this subject on the present occasion in a variety of ways. I feel very much tempted to go into it historically on account of the very magnificent collection of microscopes, of almost all periods, from the very earliest made down to the most recent; but I think it would be more useful to draw your attention to those points which are the most important in practically working with modern instruments.

Without further introductory remarks, I will first call attention to the reason of the magnifying power of a single lens. If we have a plano-convex or double convex lens, the rays proceeding from the focus are made parallel by passing through the lens, and as our eyes are only constituted to see distinctly when almost parallel rays enter them, the result is that if an object be placed in the focus, and the eye be placed on the other side of the lens, an object at that short distance can be seen distinctly. If the lens were not there the object could not be seen distinctly, unless placed at a certain distance, dependent on the length of sight of different individuals. Supposing that the distance at which an individual can see distinctly is ten inches, and that the lens has a focal length of one inch, an object clearly seen in the focus will appear as large as one ten times the diameter at the distance of ordinary vision, because it will subtend as

reflections from the sides; consequently when aqueous vapour was employed it was deposited on the sides of the tube and diminished the radiation. It seems that this is the explanation of the very large diminution in the amount of heat transmitted when aqueous vapour was employed in this tube. The celebrated Dr. Magnus employed a different form of apparatus, and also repeated this experiment, and he came to the conclusion that aqueous vapour is not so enormously absorptive of the ultra red rays as Professor Tyndall found; but in other cases Dr. Magnus's results were in complete accordance with Professor Tyndall's.

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large an angle. The magnifying power of a single lens thus depends on its bending and making more or less parallel rays that would otherwise enter the eye at a very divergent angle. We may indeed obtain a similar apparent enlargement by looking through a minute hole in a blackened card, which cuts off the divergent rays, but in this case so much light is lost, and the diffraction is so great, that the definition is bad. We thus see that the so-called magnifying power of simple lenses is due merely to the fact of their enabling us to see objects distinctly when they are very near to the eye.

Single lenses are useful for examining an object with comparatively low powers, but they do not give perfect definition unless they are only very small segments of spheres and none of the rays are very much bent. They do not bring all the rays to an exact focus, but have what is called spherical aberration. The rays of light passing near the centre are brought to one focus, those passing further from the centre are brought to a focus nearer to the lens, and those at the extreme edges still nearer. The result is that in a single lens made with *spherical* curves you never can have an object exactly in focus, since you have different focal lengths for different parts of the lens. That could be overcome by lenses with elliptical or hyperbolic curves, which would bring all the rays to one focus, but the difficulties of making them would be so great that, practically speaking, they cannot be made in a satisfactory manner. I draw your attention to the fact because the so-called spherical aberration with which we have to contend in making microscopes is a matter of very great importance, and it is very desirable that the true source of the difficulty should be known.

Various combinations of lenses have been made in order to avoid as much as possible this spherical aberration. It would occupy too much time to enter into details, but by combining different lenses the spherical aberrations may be so greatly reduced that very valuable doublet and triplet lenses may be constructed, which are exceedingly useful for so-called simple microscopes—that is to say, microscopes having only a single system of lenses. The chief advantage of simple microscopes is, that the object is seen in its proper position—it is not inverted. In dissecting and in preparing objects they are very useful, because all the motions of the hand are seen in the natural direction. We are, however, limited very much in their use

since, in order to get very high magnifying powers, the focal length must be very short, and then the objects are so very close to the lens that it is almost impossible to perform any necessary operations under them.

Independently of the so-called spherical aberration, we have to contend with what is called chromatic aberration. As I have said, the spherical aberration might be overcome if it were possible to grind elliptical or hyperbolic lenses; but even then we should still have to contend with what is called *chromatic* dispersion. When light is passed through a prism with inclined faces, it is more or less bent out of its original course; but the different rays are thus refracted to a different extent, the blue rays being much more bent than the red rays and the green to an intermediate extent. The result is that there is a different focal point for the different rays of the spectrum. When white light passes through any ordinary simple magnifying lens the focus for the blue is thus shorter than for the red. Hence, on looking at an object, you see it fringed with colours, and get an indistinct image, and you see colours that ought not to exist. At one time it was thought that the evil could not possibly be remedied; and even so great an authority as Sir Isaac Newton concluded that it could not be overcome. Fortunately, however, we have now the power of overcoming it to a very great extent. The principle on which this depends is, that the extent to which the different rays are separated by different kinds of glass—their so-called dispersive power—does not vary directly as their refractive power, and it is thus possible to combine together two different kinds of glass, so that the dispersive power of one may counteract the dispersive power of the other, and yet only partially counteract its refractive power. This is best illustrated by what is seen in compound prisms made of crown and flint glass, the relative dispersive power of which latter is much greater than that of the former. According to the angles of the two kinds of glass, prisms may be so combined that we may obtain dispersion without refraction, or refraction without dispersion, the former being a so-called direct-vision prism, through which the light passes in a direct line, but much dispersed, and the latter an achromatic prism, bending the light but not dispersing it, and thus showing objects out of the direct line, but free from coloured fringes. This is what we want in constructing

microscopes. Success depends almost entirely on the property I have described, and on compounding together flint and crown glass, in such a manner that you can take advantage of a considerable amount of refractive power, but almost entirely obliterate the dispersive power. This may be done by combining a double convex lens of crown glass with a plano-concave lens of flint glass, and when the surfaces have the proper curves, and the lenses are of the proper thickness, the dispersive power of the crown glass may be counteracted, whilst its refractive power is only partially reduced. We can thus construct lenses of moderately short focus length, in which the images of different rays are brought to the same focus, and thus can obtain by this kind of combination simple achromatic lenses.

I will now proceed to the consideration of compound microscopes, because these are in the present state of science far the most important. The characteristic principle of these consists in their having an object-glass placed at the lower end of the body of the instrument, which forms at the upper end an image of any object on the stage. This image is itself considerably larger than the object, the diameter being as many times greater as the length of the tube from the object-glass to the image is greater than the distance from the object-glass to the object on the stage. The image thus formed is inverted both vertically and laterally, and would appear perfectly distinct if it were thrown upon a white surface and examined in front, or on ground glass and seen from behind. If no such screen be placed to receive the image, it is still, as it were, formed in free space and is capable of being again magnified by another lens, or system of lenses. The principle of compound microscopes consists in their forming an enlarged inverted image by means of an object-glass, and in making it visible when near to the eye, or, so to speak, magnifying it with an eye-piece.

In the very earliest compound microscope this eye-piece consisted of a plano-convex or double-convex lens, but it was soon found that a combination of two plano-convex lenses gave a far better result. Modern eye-pieces are of two kinds. The Huyghean eye-piece consists of two plano-convex lenses—the upper one called the field-lens and the lower the eye-lens, with a diaphragm between them at the focal point of the eye-lens. The eye-lens is principally concerned in magnifying

the image, and the field-lens in increasing the field. The other eye-piece is called the negative, or Kelner's eye-piece, and has no diaphragm, and the focus of the upper lens is on the field-lens. The advantage of this is that we get a much larger field, but the disadvantage is that the definition is not so good.

Since we thus examine with the eye-piece an inverted image of the object, it is in all cases seen as it were turned upside down. This general principle of magnifying an inverted image was brought into use at as early a period as 1590, by Janssen, whose most interesting instrument is in the Exhibition, though, in comparison with our modern microscopes, it might be looked upon as little better than a toy.

The magnifying power of compound microscopes depends on both the object-glass and the eye-piece. The shorter the focal length of the object-glass the larger is the image formed in the upper part of the instrument. The final result also depends on the magnifying power of the eye-piece itself. We can thus increase the magnifying power of the instrument by having the object-glass of shorter focal length; and can likewise increase the power by using eye-pieces of shorter focal length. On the whole it is, however, better to use an object-glass of short focal length, and an eye-piece of moderate power, than to use an object-glass of long focal length and enlarging the image with an eye-piece of too great power, since we can thus utilize a greater beam of light, and obtain better definition.

The simplest form of achromatic object-glass that could be used would be a combination of a double convex lens of crown glass, with a plano-concave of flint; but though one of this kind may give a good result when the focal length is considerable, it would be impossible to obtain good definition when the focal length is small, and the aperture great. By combining three such compound lenses of different sizes somewhat differently corrected, the spherical aberration may be very greatly reduced, and the general effort much improved, since a short focus can be obtained with lenses of less curvature than when only one compound lens is used. Until the last few years most of the best object-glasses were constructed on this principle. Some modern high-power object-glasses are, however, much more complex, and consist of a combination of as many as eight lenses, nearly all of different curvature or size. The magnifying power is in many

combinations chiefly due to the lenses placed at the bottom of the object-glass, and the spherical and chromatic aberrations are corrected as far as possible by the lenses placed above them, but as I do not suppose the ladies and gentlemen here present are at all likely to make microscopes for themselves, I will not occupy time by going into all the details of construction of object-glasses. It is a most difficult subject, both in theory and practice, and the more one knows of the difficulties to be contended with in making lenses of high power the more one feels astonished at the excellence of some that are now made. Some of the high-power object-glasses, with lenses as small as a pin's head, are all so accurately finished, fitted, and adjusted, that in my opinion they are triumphs of science and skill.

In looking at an object with a high-power object-glass made perfectly correct, both for chromatic and spherical aberration, if you place over the object a piece of thin glass with water or Canada balsam underneath, the corrections are no longer strictly accurate, and it is necessary to make different corrections in the object-glass. This is accomplished by means of a screw in the collar which is made to turn round, and the lenses can be made to approach to or recede one from another. In this manner corrections can be made for the difference in the conditions due to the glass cover.

Many object-glasses are what are called dry lenses, that is to say, there is air between the object-glass and the object. In low powers this is almost invariably the case, but in some of the high powers now made, advantage is taken of what is called the immersion principle. In using such object-glasses a small quantity of water is placed between the front lens and the glass cover. One great advantage of this is that we get a much greater amount of light. Another advantage is that you can correct the lens more easily, and perhaps utilize a wider beam of light. It would be tedious to enter into the discussion that has taken place with reference to the difference in the size and aperture in dry and immersion lenses. Many most eminent authorities have differed exceedingly about this, and a most angry discussion has taken place on the subject. In any case, it appears that with immersion lenses we obtain a greater amount of light, especially that of wide deviation from the direct line of vision, which is of the greatest value in defining minute structure.

I will now proceed to say a word or two with regard to the construction of the instrument itself, and the general mechanical arrangements requisite to hold the optical part and the object under examination, and for illuminating it properly. The Exhibition contains as simple a compound microscope as is possible, made by Janssen, the first ever constructed. There is no stage and no mirror to reflect the light, merely a simple lens for object-glass and a single lens for eye-piece, held in tin tubes. There are also many kinds of most interesting microscopes, of nearly all periods down to the most modern, but I will allude to only a few to illustrate some particular points. Passing down from the one just named by Janssen, we come to those forms of instrument which are especially characterized by very large eye-pieces of two lenses on Huyghens's principle. As an example, I may refer to the two microscopes made by Marshall in the commencement of the eighteenth century, the eye-pieces having field-lenses of two inches in diameter, which makes the instrument look most remarkably stout. Few microscopes have been made larger or more complex than those by Martin in the middle of the eighteenth century. One of these in the Exhibition belongs to the Royal Microscopical Society, and it was thought to be the only one in existence, but another has turned up which I had the opportunity of putting together; I must confess I had very great difficulty in finding out where the different things fitted. There is almost every movement that any one could possibly devise, and a great many of them for the practical work of the present day are not only useless but far worse than useless. Then again the construction is exceedingly curious. One tube goes into another for only about one-eighth of an inch; and there is scarcely a firm joint in the whole apparatus. There are movements of nearly every decription; you can move the body of the instrument up and down, from side to side, and backwards and forwards, but there is scarcely a single joint which one could call good. These instruments may be looked upon as the opposite extreme of that by Janssen.

Leaving the microscopes of mere historical interest and coming down to modern times we have examples of the most simple and complex arrangements. Some are of the very simplest form, more in vogue on the Continent than in this country; so constructed that you have not the power of

inclining the body, but have to look straight down. There is a disadvantage in that because the tear of the eye runs over the pupil, and you do not get such good definition as when you can incline the microscope to any convenient angle, so that the tears may not remain in front of the pupil. Some have the stage as simple as it is possible to be, the body is a simple tube, and you get a coarse adjustment by pulling it up and down. There is a reflector below the stage, but scarcely anything which is not absolutely requisite.

Many of the microscopes in the Exhibition have various excellent and valuable movements. It is very important to have no vibrating motion in the tube, because if you are in a place where there is the least tremor, and the tube is able to move in the least degree from side to side, the object is seen to vibrate in the most inconvenient manner. Some of the early microscopes are for this reason totally unfit for difficult observations. I think it a disadvantage to have the body move in any direction but up and down, unless the instrument is very well turned out of hand. If it is made to perfection the power of turning on one side may be very good for particular purposes. An excellent stand for holding the tube is the so-called Jackson model. The body, the stage and the sub-stage, are all firmly held by one solid piece of metal, and must therefore vibrate together in such a manner as to be of little importance. The body slides up and down on a long groove with rackwork, and there can be no more secure means of preventing lateral movement, because it is held so firmly for such a distance.

Proceeding to the stage, we have some of the most simple form, and others with very complex movements, so that you can move the objects up and down or from side to side by the milled heads, or can rotate the whole round quickly or slowly with the screw, the object remaining all the while in the centre of the field. I might say much more on these various arrangements if time would permit, but cannot, I think, do better than express my general conclusions in a simple form. I would strongly insist on the desirability of having every accessory movement well made or not made at all. Whatever movements there are should do only just what is required, and should admit of no other motion, and this can only be accomplished by good and careful workmanship. If you do not wish to be at the expense of good workmanship you

should have a microscope of simple construction. It is far better to have one of the simplest form with very few movable parts, than to have a microscope with all the movements I have described badly executed. If you want a cheap microscope, have a simple one, but do not get a cheap microscope which has a great many movements which work irregularly, give tremor, or throw the object out of focus.

I now come to the practical use of the instrument. Assuming that you have a microscope of a satisfactory kind, when you come to use it success depends as much on properly understanding how to illuminate the object as on the quality of the instrument itself. You may have a splendid microscope, but unless you illuminate the object properly, you will see nothing, for the simple reason that there is nothing to see. I think I could not do better on the present occasion than deal somewhat at length with this part of my subject, since it is of such great importance in the practical use of the microscope. First of all then, I will draw your attention to the surface illumination to which we must have recourse in examining opaque objects, and which may sometimes be used with advantage in the case of objects which are not opaque. With this kind of illumination very much depends on light and shade, as in the cases of objects which we see with the naked eye. We all very well know that if you have light thrown directly on a wall it may appear to have a perfectly uniform surface; but if the light be thrown very obliquely all the irregularities of the plaster, blisters and such things, are made very conspicuous. This illustration serves to show that if you have the light thrown at a certain angle you can see no character whatever; but if thrown at another angle the surface structure may be seen as well as could be desired. In some cases if you were to throw the light at too oblique an angle certain structures might be hid by the shadows of other greater irregularities, so that for each particular object it is very important that you should throw the light at particular angles, and in examining an unknown object it is desirable to try the effect of light of various angles of obliquity. There are various ways of obtaining this surface illumination. We can have a bull's-eye condenser throwing the light at various angles from the side, or we can use a silvered parabolic reflector, the first of which was made for my own use in examining iron and steel. One advantage of these parabolic reflectors is that

the shadows appear true. When you examine an object with a bull's-eye condenser throwing light from one side to the other, you instinctively know which way the light is coming; and when you look at an object, the object being inverted, you see the lights and shadows all the wrong way. On the contrary, in using these parabolic reflectors the light is thrown from the opposite side, and the image being inverted by the microscope, when you look at the object the lights and shadows appear natural, as they would do if you looked at the object without a microscope. That is certainly a great advantage; but there is this disadvantage in a fixed reflector, that you cannot alter the angle at which the light is thrown. There is, however, no reason why we should not so modify these reflectors, and so arrange them with reference to the source of light, as to partially overcome this objection. We have also what are called Lieberkuhns, which are sections of a concave silvered mirror. The light is sent up by the mirror at the bottom of the microscope, and reflected back on all sides of the object by this concave mirror to the object, which is thus equally illuminated in all directions. There is a hole in the centre of the mirror to admit the light to the object-glass, so that there is no absolutely vertical light. In some cases it is an advantage thus to have the light equally in all directions, since it enables you to see certain structures well. This is, however, sometimes a disadvantage, since it gives no shadows, but this could be overcome by stopping out portions of light from one side or the other. With a Lieberkuhn you usually do not see structures revealed by light and shade, but see those due to such difference of texture as reflect light in a varying manner. If the object is very small or transparent you must use what is called a dark well put immediately under it, so as to prevent the light passing at the side or through it. With this illumination perfectly transparent substances appear black, but if any part of the object be full of little colourless granules they look white, and if coloured the colours are well seen. These kinds of surface illumination cannot be used with very great advantage, except with moderately low powers, but for them they are often everything that could be desired.

The bull's-eye, parabolic mirror, and Lieberkuhn show objects illuminated on the upper side by more or less direct reflection; but another class of illuminators which like them give a

naturally dark field, show the structure of the object by light thrown on them from below, bent out of its normal course by more or less oblique reflection and refraction. This may be accomplished in various ways. The light may be thrown on the object by the mirror from one side in such a manner that if no object were on the stage it would not pass at all up the body of the microscope. But if the object placed on the stage is granular, or has little facets and other irregularities of structure, the light would be so bent as to pass up to the eye. This may be accomplished also by means of two or three different kinds of illuminators, such as Amici's prism, Mr. Wenham's reflex illuminator, and his parabolic reflector, or by a large lens of short focal length, with a central black spot to stop out direct light. All these different kinds of illumination depend upon the light being thrown so obliquely from the under side that it will not enter the object-glass unless it be turned out of its course by the reflection or the refraction of the various parts of the object. One great advantage is that the field of the microscope is dark, and the eye is not in any way distressed by the light which comes in at the sides. Certain kinds of structure are also seen to very great advantage, especially with binocular instruments. I am inclined to believe that the further development of this kind of illumination would yield better results than can be obtained by the ordinary method of illumination by transmitted light.

Another kind of illumination which is on the whole more common, is where the light is reflected directly up the tube of the microscope, so that if no object be placed on the instrument the field of the microscope is filled with light, and if any object be there it is seen by variation in the intensity or colour of the light. I wish to draw your attention to one or two points connected with this kind of illumination, because, in my opinion, its further development with certain modifications which I believe have not been carried out, will perhaps enable us to overcome certain difficulties which at present stand in our way. Much may be learned by the study of mineral structures, since in the case of crystals and of solid portions of glass and other analogous objects, we know what their character is, whereas in the case of minute organic structures we have rather to infer what is their structure from what we see. Therefore in

forming some general idea of illumination, I think we may learn a great deal by studying what we see in small crystals, and in inorganic bodies of pretty well known form. If light be sent straight up through a small crystal, having parallel faces terminated by an oblique plane, it will pass up directly in one part and be bent and thrown aside altogether in the other, and in looking at such a crystal with the microscope, you might see a black edge with perfect definition, but if you were to throw the light obliquely with a condensing lens of considerable aperture, the light might enter at such an angle that it would be bent and pass straight out, and if the edge of the crystal were absolutely perfect you would see no dark band at all. When thus illuminated, the crystal might be quite invisible.

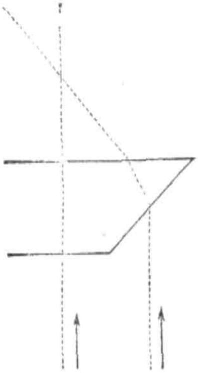

FIG. 1.

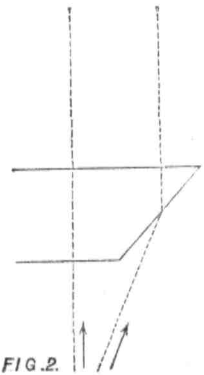

FIG. 2.

My meaning will be better understood by means of the following rough illustrations.

In Fig. 1 the light is supposed to be all parallel, and is bent quite out of the line of vision by the oblique end of the crystal, whereas in Fig. 2 the light is supposed to be so very variably divergent, that some of it can enter the object-glass after passing through the crystal, both where the sides are parallel and where they are inclined. Such a case clearly

shows the importance of regulating the aperture of the condensing lens used for illumination, since with one aperture the edge of the crystal would be shown by a dark band, and in the other might be invisible. When low powers are used the most convenient condenser is a moderately large plano-convex lens of short focal length, or two so combined as to give less aberration, but in using high powers an achromatic condenser of more complex structure, short focus and large aperture, is very desirable. Even with rough and imperfect mechanical arrangements I have been able to see sufficient to convince me that even with achromatic condensers it is a very great advantage to have two different diaphragms, one to modify the divergence of the light, and the other the size of the beam passing up through the sub-stage. By using only light of high angle of divergence, and a small opening in the sub-stage diaphragm, some objects are seen to very great advantage. Such an illumination is impossible with condensers constructed in the ordinary manner, with only one diaphragm placed in such a position that it gives imperfectly both these effects combined.

You must remember that the light passing near the centre of a condenser is inclined at a small angle to the line of vision, whilst that which passes through the exterior zone is much more divergent. Hence the light from the outside part of the condenser may pass through certain parts of the object, but if you stop off the outer zone of the condenser no light may pass through those parts and you may see a well defined dark edge in such a crystal as I have alluded to. By making the opening wider and wider, and thus allowing more and more divergent light to pass to the object, you may as it were obliterate the crystals on the stage. It is sometimes important thus to be able to make the light pass through an inclined edge. For instance, I have a beautiful crystal of sapphire with a fluid cavity containing liquid carbonic acid. It happens to lie in such a position in the crystal that if I use light of a moderate angle of divergence, the fluid cavity is completely hidden, because no light passes through where it lies, but on increasing the aperture and the obliquity of the light the dark shadow disappears, and the fluid cavity is perfectly well seen by transmitted light. The possibility of either seeing the object or not seeing it at all, thus entirely depends on knowing

how to modify the angle of divergence of the light used for illumination. If you had light passing through parallel, like that reflected from an ordinary direct mirror, you would see a broad dark band, and would not have the remotest idea that such an interesting cavity existed; but if you used as a condenser, an almost hemispherical lens, you would then see the fluid cavity to perfection. The use of such simple single or compound condensing lenses of large aperture is not common, but they enable us to study certain objects in a very satisfactory manner, since with appropriate stops the angle of divergence of the light is so very much under control. This principle may also be brought to bear in another class of objects. There may co-exist two perfectly independent kinds of structure, which with ordinary illumination may so far unite together as to produce a general appearance of a very misleading character; but by varying the divergence of the light or the size of the sub-stage opening, first one and then the other structure may be made invisible, whilst the other is seen to great advantage by itself.

Another interesting illustration of the importance of the angle of divergence of the light is furnished by the little spherical cavities met with in amber, some filled with gas and some filled with water. By proper illumination you can see these very well, but I find that the character of the object and the illumination depends on a set of conditions which I believe have not attracted any attention. In looking at such spherical cavities in minerals, which I choose as a type because they are so simple, and we can understand them perfectly well, or in looking at a spherical cavity or a bubble in glass, you see first a black outline and then a white bright centre. This bright centre is nothing more than the image of the opening at the bottom of the condenser seen out of focus, and by a little modification in the focus you can see very distinctly anything special in its character. My meaning will be better understood by giving a few illustrations of what may be seen in examining the gas cavities, which are so extremely numerous in some specimens of amber. When the opening in the sub-stage diaphragm is large, you see a broad clear space and a relatively narrow dark band, extending inwards from the external outline of the cavity, as shown by Fig. 3; but on making the opening in the diaphragm small, this black zone closes in, and there is only a small bright

centre, as in Fig. 4, which you can see distinctly is nothing but the image of the hole in the diaphragm, seen more or less in focus according to the adjustment. If you take a diaphragm with a central stop, you see what looks very strange, viz., a bubble or spherical cavity with a central black spot, from which proceed the two small arms, as shown by Fig. 5. This appearance is thus manifestly due to nothing but the image

FIG. 3.

FIG. 4.

FIG. 5.

of the opening in the diaphragm, the central stop, and the two arms which support it. These very simple facts thus clearly prove that what might easily be mistaken for structure may be merely the light and shade depending on the kind of illumination made use of. This way of viewing the subject has occurred to me only quite recently, and I feel persuaded that an arrangement which gives us the means of limiting the obliquity of the light and also the size of a more distant opening is very useful, and would be equally applicable in the case of rods, and minute fibres, and such kinds of structures as are commonly met with in organic bodies. In the case of the markings on some diatoms, we do indeed see all the appearances that would be due to a vast number of small lenses arranged side by side; and, as in the case of the above-described cavities, we may so modify the illumination as to show the external outlines of these bead-like lenses, or to see merely a central black spot in each of them. As a general rule, however, the curvature of the surfaces of organic bodies are seldom sufficiently regular to give distinct images of the openings in the sub-stage diaphragm, but yet there can be little doubt that the lights and shades are to a very great extent due to a similar cause, and that as far as this is concerned they might be looked upon as very much distorted and irregular spherical or cylindrical lenses. In any case, much more may be learned of their true form by carefully observing and discussing the changes that take place, on varying the divergence of the

light and the diameter of the sub-stage opening, than can be actually seen with any one single illumination. What I am anxious to do is by an inductive process to be able by further examinations to form something like an intelligible explanation of the cause of the lights and shadows that we see in objects as examined by transmitted light, and in order to do this it is most important to be able to vary the illumination very much, and at the same time to know exactly what its true character is—whether slightly or very divergent, or proceeding from a small or wide sub-stage opening.

Independently of objects whose structure is shown by lights and shadows in the way I have alluded to, we have many which are seen by the difference in their colour, and then it is only a question of illuminating the surface, or transmitting light through the object. In sections of rocky substances, the different constituents, black, red, brown, or otherwise, are seen at once by their colour, independently of any light or shade. This method of distinguishing objects by difference in colour is extensively employed in studying organic structures, by using a staining on which they act like mordanted textile fabrics. Various staining materials will thus combine with certain constituents and colour them, and have a very slight effect on others. In that manner you recognise the different constituents of what otherwise would appear like almost homogeneous structure.

Another important question connected with the microscope is illumination by means of polarised light. I must take it for granted that you all know what polarised light is, since time would not permit me to describe its characters. One principal point is, that polarised light has different properties in different directions. We can sometimes make use of it very effectively independent of an analyser, by having a polariser under the stage of the microscope and illuminating the object with polarised instead of with ordinary light. Polarised light, when the plane of polarisation is inclined in certain directions to an edge of a crystal or other inclined surface, may be to a great extent reflected and not transmitted to the eye in cases where the amount reflected would be very much smaller if you used ordinary light. In this manner you may define crystals or other transparent objects mounted in Canada balsam, which is of so nearly the same refractive power that they can scarcely be seen with ordinary light.

In such cases the use of polarised light without an analyser may be very useful. You use it simply as light, and rotate the prism so as to get the plane of polarisation in different directions. The more common plan is to use it with an analyser placed generally over the eye-piece. By certain adjustments, when no object is on the stage, the whole field of the microscope is dark; but if you place under the microscope objects which have more or less powerful double refraction, they depolarise the light and appear either white or coloured, or invisible, according to the angle at which they are placed in relation to the plane of polarisation. In order to study these effects, either the object or the plane of polarisation must be rotated. Time will not permit of entering further into it, but I may just draw your attention to one or two illustrations. Some objects may show little or no structure when examined with ordinary light, but by using polarised light various dark markings and colours may be seen, which vary as you rotate the plane of polarisation. By careful induction you may form a very accurate opinion as to the kind of structure before you. Thus, if the object give a well-defined black cross rotating with the plane of polarisation, it indicates that small crystals radiate uniformly from a single centre; whereas, if there be an irregular varying distribution of dark patches shading off into bright or coloured portions, all changing gradually as the plane of polarisation is rotated, there must be an irregular grouping of imperfectly radiating crystals. On the other hand, if on rotating the plane of polarisation the object becomes uniformly dark and bright, you know it is a portion of one simple crystal. A great many other most interesting facts may be made out in that manner by using polarised light. Such general conclusions are more simple and obvious in the case of mineral structures, but are by no means confined to them. Much may thus be learned respecting the arrangement of the mineral matter in the various kinds of calcareous shells and other organisms, and I cannot but think that much remains to be learned even in the case of more purely organic structures.

When we use very high powers—1,000 linear, or upwards—and employ object-glass of very short focal length, we must contend with another class of difficulties quite distinct from any I have hitherto mentioned. In using very high powers

we are approaching, if we have not already passed, the limit that is allowed to us by the physical characters of light itself. When we have to deal with objects from $\frac{1}{40000}$th to $\frac{1}{80000}$th of an inch in diameter and still less, we come to the length of the waves of light, and must contend with difficulties due to the fact that we are trying to examine objects which are small in proportion to the waves of light. It is a question, if it be not already certain, that even if the optical contraction by transmitted light were in every respect perfect, we could never distinctly see the true outline of objects less than half a wave-length of light, though their mere presence might be recognised. It may seem strange to say it, but the fact appears to be that the physical constitution of light itself is too coarse to enable us to see all that we could desire, and could see if light were of a more refined character. A great cause of this difficulty is what are called interference fringes. If you have an object which ought to look like one dark line, and examine it with a high power, you find dark and coloured lines on each side, and in certain cases these are almost as distinct as the object itself. There appears to be little doubt that certain structures have been described by microscopists which look as distinct as if they were real markings, though due to nothing more than interference fringes. *Such an inaccurate interpretation is entirely due* to the fact that light, so to speak, breaks down when we try to examine objects which are small as compared with the waves of light. It would occupy a very long time to enter into the full particulars, but I may say that one of the principal means of overcoming this difficulty is by increasing the aperture of the lenses. The width of these interference fringes depends to a considerable extent on the angle of divergence of the light used in illumination, and *passes into the object-glass*, and by *increasing* the aperture so as to bring into play more and more oblique rays, the size of these fringes is lessened, and thus you improve the performance of the instrument. By theory the defining power ought to vary as the chord of the angle of aperture, and microscopists working practically without any regard to the undulatory theory of light had come exactly to the same conclusion. With dry lenses you do not get so good result as with the immersion lenses; since with the latter the interference fringe would be only three-fourths

the width of those in the case of a dry lens of equal aperture, and therefore, other things being equal, an immersion lens may be said to have four-thirds the defining power of a dry lens. In all these kinds of examinations, in order to fully utilise the capabilities of the object-glass, the light passing from the object ought to be of the same angle of divergence as that which can enter the object-glass. With an object not covered you might obtain greater divergence, if light could be made to pass in at a greater angle. However, in making object-glasses of very wide aperture we come across great difficulties. By increasing the size of the aperture we increase the difficulty in making them correct in other respects, especially in correcting the spherical and chromatic aberrations. Another practical difficulty is that the object-glasses come down so close to the object, and it is impossible to see anything through a thick cover, or when some distance within a transparent portion of the object itself, as is so often the case in studying the microscopical structure of minerals, or the crystals enclosed in blowpipe beads. I very much fear that it is only too true that if you improve object-glasses in one respect, you make them worse in another. Those qualities which are necessary for one purpose are unnecessary for another; and in my opinion we ought to have object-glasses constructed for the particular kinds of work we wish to use them for. If we wish to define very close markings on thin flat objects, like diatomaceæ or the minute striæ of muscular fibre, we must have a very wide aperture, even if the object is then so very close to the object-glass that the range of vision is too small when we come to study other kinds of objects. For instance, in some cases, with the very best object-glasses of a very high angle, which would give the most splendid definition of test objects, you might not be able to see other objects you wanted to examine, because you could not get at them. They would be beyond the focal point when the object-glass touched the cover. In such a case, with only moderately fine structures, an object-glass of much less cost and smaller angle of aperture might enable you to see all the necessary detail, and you could get at the object, even when under one-tenth of an inch of Canada balsam, or other transparent substance, where you could not have seen it at all with a lens of high angle and of the same magnifying power. I am, therefore, inclined to believe that

the kind of object-glass ought to depend on the kind of work we wish to do. If we want to examine the very minute structure of thin flat objects we must use one kind, and if we want to look at things through a considerable thickness of water or glassy matter, and to study various irregular objects met with in original research, we must have glasses which do not approach too near the object, even if the angle of aperture and the definition of very close markings are by this means somewhat diminished.

In connection with the visibility of very minute objects there is another interesting point. If in order to define objects that are very close together you increase the angle of the divergence of the light, you then bring into play another principle. I have already described how you may thus obliterate the object, by destroying all difference in shading. We come then to this dilemma, that if the aperture is small we cannot see the object, for one reason; and if we have the aperture large we cannot see it for another reason. In one case the light would break down, and in the other the object would not possess any character which would enable us to see it, and thus with these very high powers we come across a state of things which makes it exceedingly difficult to go much beyond what we have obtained. In fact, I am much inclined to believe, that as far as the size of the object is concerned, we have pretty nearly got to our limit. I hope I may be wrong, but I am very much afraid, that except under special conditions, we can never see objects of as minute a size as would be very desirable in studying certain characters of natural history, and that our powers are limited by the constitution of light itself. Perhaps something may be done to increase our power by not using simple transmitted light.

This lecture would be incomplete if I were not to say something about the difference between the monocular and binocular microscopes. The disadvantage of monocular instruments is of course that we use only one eye. We injure one eye with using it too much, and the other by not using it enough, and we also do not have the stereoscopic effect. These disadvantages led to the desire to contrive some method by means of which we could make use of both eyes. In the Exhibition you may see a magnificent collection, lent by Mr. Crisp, including, I believe, every kind of binocular instrument that has been contrived. Some of these are so uncommon

that the leading manufacturers do not know of their existence. Amongst these various instruments may be named those constructed in the manner proposed by Nachet, Wenham, Stephenson, Holmes, and Ahrens, but by far the most usual are those made on the plan contrived by Mr. Wenham. There is a small prism which can be pulled out or pushed in, extending half over the object-glass. This prism is so constructed that the light enters vertically, is twice reflected, and passes out vertically, so that there is no dispersion, and the beam remains colourless; but by the second reflection the light is sent up the oblique body of the microscope at such an angle that the upper end of the two bodies are nearly at the distance of our two eyes. The light which passes through the other half of the object-glass not covered by the prism passes up the direct body of the instrument.

One great advantage in this system of Mr. Wenham is that the light which passes straight up one of the tubes is not in any way influenced by the prism; and the definition is therefore unimpaired. The definition of that half of the light which passes through the prism is somewhat impaired, but this does not signify very much, since the vision of the left eye is mainly important in enabling us to distinguish differences in the level of different parts of the object, and that of the right eye gives good definition of the minute detail. Another great advantage of this method is that you can at once convert the instrument into a monocular microscope. In some cases it is very desirable only to use one tube, and this you have a means of doing by pulling out the prism. This is no doubt the chief reason why this form of binocular has been so much more commonly employed than any other, although I am by no means certain that other forms are not to some extent better, at all events when high powers are used.

I have been informed that Mr. Stephenson's arrangement gives exceedingly good results with very high powers, which the other one will not. In it there are two truncated rectangular prisms close behind the object-glass, so placed that they reflect the light up the two bodies, both inclined to the direct line of vision. By this reflection the object is inverted laterally, and is still further inverted longitudinally by a larger prism, so that finally the object is seen in its natural position, being inverted by the object-glass and re-inverted by the prisms. This instrument has the advantage of giving good results with

high powers; it acts as it were like an erecting glass, and when inclined at a convenient angle the stage is horizontal, which is very convenient in examining objects in liquid. It is thus a binocular erecting microscope, as Mr. Stephenson calls it. A great many other forms have been proposed, which can be seen in the Exhibition, some having one advantage, and some another, whilst perhaps we may say that some have none at all.

In conclusion, I may mention the application of spectrum analysis with the microscope. If you want to ascertain more exactly the nature of the light transmitted by any coloured object, you can do so by this means. In the eye-piece arrangements there are many things which are required only for special purposes, and but for that the instrument might be made much more simple. It is constructed for carrying on all kinds of inquiries in this subject, and for measuring and comparing spectra together side by side. You can take out the ordinary eye-piece and put in the spectrum eye-piece; or with a binocular microscope you may see the object in its natural form with one tube, and see the spectrum with the other. I have for some years chiefly used my binocular arrangement, which for working is undoubtedly the most convenient, but some parts of its construction are rather unusual and the makers do not particularly like making it, because they do not exactly understand some of the necessary details.

As I said at first, the entire subject of the microscope is so great that it is quite impossible in the course of an hour to do more than give a hasty glance at some of the leading features. In this Exhibition we have the means of studying all that we can desire. We have the simplest and the most complex, the earliest and the most recent of microscopes; and I may say I feel tempted to spend many hours in going into the details, in order to thoroughly understand the merits and demerits of these magnificent instruments. I hope that what I have said may enable you to better understand their construction and be of some practical use. I am quite sure that, in the present state of our subject, what we want is to know something of the general principles of the construction of instruments; but still more to understand the very great importance of proper illumination and of using object-glasses of a proper kind. I must also say, that it is most important to regulate the power which you use for particular purposes. It is mere child's play to try

to make a thing look bigger than it is, and to say you have magnified it so many times. What we want is to use the magnifying power that will show to the greatest advantage each particular structure which we want to examine. You should use the lowest power that will enable you to do that, since it is often very important to see as much as possible of the object, so as to understand the bearings of one part on the other, and for this you must use low powers; but when you want to define certain very minute structures, you must use high powers. You should always try first a low power and see what that shows, and then examine with a higher power in order to resolve structures that cannot be resolved with lower powers; but never use high powers unless there is some evidence of such minute structure as cannot be properly seen with low.

ELECTROMETERS.

TWO LECTURES.

BY J. T. BOTTOMLEY, M.A., F.R.S.E., DEMONSTRATOR OF NATURAL PHILOSOPHY IN THE UNIVERSITY OF GLASGOW.

LECTURE I.

WHEN a difference of electric potentials exists between two points it gives rise, or it may give rise, to one of two effects: an electric current may be produced, or there may be an exhibition of electrostatic force. Either of these effects may be employed for measuring differences of electric potential, and hence we have two classes of instruments for this purpose. Thus we have galvanometers, electro-dynamometers, voltameters, in which the difference of potentials between two points is inferred from the measurement of the current passing between those points under known circumstances. On the other hand, we have electroscopes and electrometers which indicate and measure differences of electric potential by means of the effects of electrostatic force; that is by means of the attractions or repulsions observed between electrified bodies. An electroscope merely gives an indication of the existence of the difference of potentials; an electrometer, properly so called, measures the amount of the difference. The first requisite of an electrometer is to furnish numbers, by the scale readings, which are proportional to the difference of potentials between the points tested. From these numbers we must

afterwards deduce numbers giving differences of electric potential in absolute measure, in accordance with the system of absolute measurement now adopted in every branch of physical science. This I will endeavour to explain more fully a little later.

The first electrometric measurements were made by Coulomb with the celebrated torsion balance. I have here an instrument similar to that which he used.[1]

The torsion balance was first devised by Mitchell, who used it for the measurement of the force of gravitation between two small bodies. Cavendish also employed it for the same purpose. Coulomb, however, independently reinvented the torsion balance, and, as a preliminary, investigated with great care the laws of torsional elasticity for the suspending fibre. He then used the balance for determining the laws of electric attraction and repulsion; and afterwards employed it for purely electrometric purposes.

From the "torsion head" at the top of this tall glass tube which surmounts the main body of the instrument there hangs a vertical wire or glass fibre. Coulomb used a fine silver wire. A fine glass fibre, as used by Faraday, was first proposed by Ritchie, and is always employed now. To the lower extremity of the fibre is attached a very light horizontal bar or lever of shellac, which carries at the extremity of the longer arm of the lever a small pith-ball well gilded, and at the other extremity a counterpoise; and, in the instruments of Coulomb and Faraday, a vertical disc of paper, or a slip of tissue paper hanging down vertically, to "damp out" vibrations and bring the torsion arm quickly to rest after disturbance. The case of the instrument is, as you see, a glass cylinder covered with a circular glass plate, into which is cemented the glass tube that carries the torsion head. Round the glass cylinder and on a level with the torsion arm there is pasted a scale divided into degrees. By means of this scale the position of the torsion arm is read off.

In the circular glass cover of the cylindrical case a circular hole is cut It is for the purpose of introducing what I shall call the "carrier ball." The carrier ball is a

[1] The diagram represents Coulomb's original instrument.

little pith ball as nearly as possible of the same size as the ball at the end of the torsion arm, and also carefully gilded. It is fixed to the extremity of a fine rod or stem of shellac, which is of such a length as to bring the centre of the carrier ball, when in position, precisely to the level of the centre of the torsion ball; and there is a proper geometrical arrangement for placing the carrier ball again and again

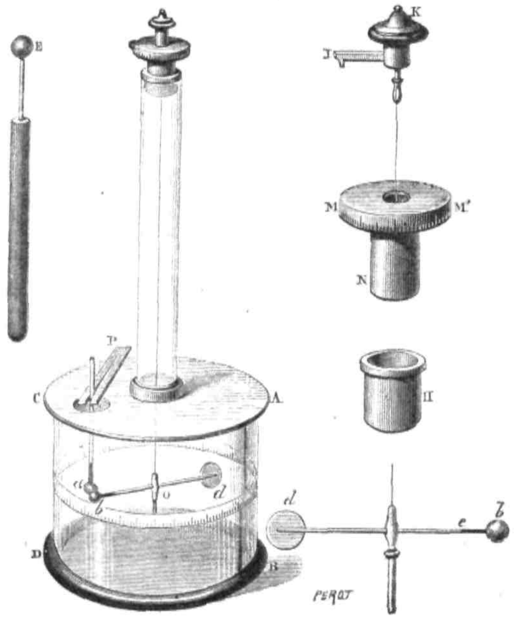

FIG. 1.

in precisely the same position, with its centre, let us say, at the same distance from the centre of the circle in which the torsion ball moves as is the centre of the torsion ball, and with one side in contact with the torsion ball when the middle of the torsion ball is opposite to the zero of the scale upon the cylindrical glass case. Lastly, as to the torsion head. The cylindrical piece, H, is cemented into the

vertical glass tube at the top. The disc, M M′, graduated at the circumference, is supported on the tube N, which fits into the cylinder H. K is a button which fits into the hole in the disc M M′. The glass fibre which carries the torsion arm is attached to the button K; and there is also an index I, which indicates on the graduated scale of M M′ the angle through which the button K has been turned with respect to it.

In Coulomb's ordinary method of using the instrument, the torsion ball is completely diselectrified. The index I is put at zero on the torsion head. The tube N, carrying, as I have explained, the scale button, and fibre, is turned round so that the middle of the torsion ball shall be at zero on the lower scale. The carrier ball is now electrified and put into its place, and the torsion ball comes in contact with it. The charge is thus divided between the two balls —halved if the balls are exactly equal and similar—and the torsion ball is repelled from the other. The displacement of the torsion arm is opposed by the torsion of the fibre, and finally we have equilibrium between the couple, as it is called in dynamics, due to the repulsion of the balls acting at the end of the torsion arm, and the couple due to torsion. By turning the button K of the torsion head, the torsional couple can be increased, which will have the effect of forcing the torsion ball nearer to the carrier ball.

Coulomb, as I have said, first undertook the determination of the laws of electric attraction and repulsion. He began by examining very carefully the laws of torsional elasticity of the wires that he employed, and established the important law that the torsional couple, for a given wire, is in simple proportion to the angle of torsion; or, in other words, that if a certain couple be required to turn the lower end of the wire through a certain angle relatively to the upper end, the couple required to turn it through twice that angle will be double; the couple required to turn it through three times that angle will be triple of the first, and so on. He tested this law to high angular displacements, and found that it holds with great exactness unless the torsion is so great that the wire receives a permanent set, and when released from torsion does not return to its original condition.

The laws of torsion being established, the first experiments of Coulomb were on the force of electric repulsion at different distances. The balls being electrified as I have explained, the torsion ball takes up a position in which the repulsion couple is balanced by the couple of torsion. The distance between the balls was determined from reading off the position of the torsion ball by means of the scale round the glass cylinder. By a simple trigonometrical calculation the distance is deduced from this reading. The torsion head was then turned, so as to reduce the distance, which was again measured in the same way, and the torsion couple at this new distance was again determined. The torsion couple required to equilibrate the repulsion between the balls at different distances, the charge being supposed to remain unaltered, was thus obtained. By these experiments, the results of which, however, required correction for the inevitable loss of charge during the experiment, Coulomb obtained his well-known law of the inverse square of the distance.

This law, I must remark, requires careful exactness in its statement. Through a complete misunderstanding of it Sir W. Snow Harris was led to experiments to disprove it. A proper statement is, that if the two quantities of electricity are placed upon balls which are so small that the diameters of the balls are insensible in comparison with the distance between their centres, the force of repulsion is inversely proportional to the square of the distance between the centres of the balls. Even this statement must be taken with limitations, because the effects of induction prevent the experiments from showing precisely that law. There are, however, other proofs, partly given by Coulomb, partly by Cavendish, which establish the truth of the law with minute exactness.

The mathematical theory for a particular form of torsion balance, taking induction into account, has been given by Professor Clerk-Maxwell in his great work on electricity and magnetism.

The next part of Coulomb's investigation consisted in determining the effect of electrifying the balls with different quantities of electricity, and to do that he used an ingenious device. Electrifying the carrier ball, and bringing it to the electrometer, the movable or torsion ball received its

charge. He now removed the carrier ball, and touched it with an insulated ball exactly equal to it in dimensions; he was thus able to halve the charge of the carrier. Putting it back, he tested the force. Again the charge of the carrier was halved, and again he determined the force, and so on. The charge on the torsion ball was then altered, and a new series of experiments proceeded with. By a series of experiments of this kind he found that the force between the two balls is proportional to the product of the quantities of electricity on them, the distance remaining the same; and combining the latter with the former law, he showed that the force is proportional to

$$\frac{m \times m'}{d^2}$$

where *m* and *m'* are the charges of the balls, and *d* the distance between their centres. But in stating this law you must be careful to mention the limitations as to the dimensions of the balls and as to effects of induction to which I have just referred.

The experiments that I have been speaking of were not so much electrometric experiments as experiments on the fundamental laws of electricity; but the next experiments of Coulomb were of a purely electrometric character. They were for the purpose of determining the distribution of electricity over variously shaped conductors. Except in certain simple cases, the mathematical problem of determining the distribution of electricity over a conductor is extremely difficult. Poisson had, however, worked out the case for two equal spheres in contact, assuming the truth of Coulomb's law of the inverse square of the distance. Now it is plain that nothing could give a better test of the truth of the law than the comparison of the distribution given by the calculation of Poisson with the distribution determined by experiment.

Coulomb used for these experiments another instrument, slightly different from the carrier ball, namely, the proof-plane, which consists simply of a very small piece of thin metal insulated on a thin arm of shellac, which is applied to the surface of the conductor to be tested, and then carried away from the conductor. It is then brought to

the torsion balance and placed in position, and the amount of electricity that it possesses is determined. The theory of the proof-plane shows that when the proof-plane is applied to the surface of the body to be tested, it, as it were, carries away a small portion of the surface of the body with its electricity upon it. While the proof-plane is in contact with the body it forms a part of the surface; and when it is lifted normally from the surface of the body with which it is in contact, it is practically a small portion of the surface of the body itself that is carried away. Coulomb, by an error, supposed that the proof-plane carried away as much electricity on *each* side as corresponds to the space it touched upon. To correct his results for this error is, however, easy.

Taking, then, the case of the two spheres in contact worked out by Poisson, and the case of an ellipsoid, for which Coulomb himself worked out the distribution mathematically, he found, on determining the distribution by experiment, as close an agreement with the theoretical distribution as could possibly be expected.

By far the best proof that we have of the exactness of the laws of electric attraction which I have just stated is an indirect proof. It was first pointed out by Cavendish that if the law of the inverse square of the distance be true it follows as a consequence that the whole of the electricity upon an electrified closed conductor must reside at the surface of the conductor. The exactness of the law will then be tested by finding whether or not the whole of the electricity does reside at the surface of such a conductor. Cavendish himself undertook some admirable experiments to test this question, and some of the most important of Faraday's electrostatic researches were devoted to it.

One of the remarkable experiments of Faraday was to place himself within a chamber—a twelve-foot cube—constructed of light material and covered with tinfoil, taking with him electroscopes of the most delicate construction to examine whether any electrification could be found in the interior while the chamber, which was insulated, was electrified; but while the whole power of the electric machines of the Royal Institution was turned on, so that from every part of the outside of the chamber flashes and brushes of electricity were rushing off, he could find

no trace whatever of electrification on the walls or elsewhere within. The most refined modern experiments lead to the same result; and it is evidence of this kind that enables us to regard the law given by Coulomb as holding with extreme exactness.

In connection with experiments of this kind Faraday used the torsion balance and made great improvements in it. He used the torsion balance in those great researches in which he investigated the curved lines of force, and worked out the theory of induction, and proved that electrification by induction is not what it was supposed to be—action at a distance—but that it is electric disturbance transmitted by means of contiguous particles of the di electric, or insulating medium, between two electrified bodies One of the modifications that Faraday made in the torsion balance was the protection of the movable parts of the instrument from external influence. He showed that the indications of an instrument such as the torsion balance, or common gold-leaf electroscope, are perfectly untrustworthy unless the most careful attention is paid to this matter; and it is curious that though it is now forty-five years since Faraday pointed this out, instrument-makers have failed, except in very few cases, to give any attention to his warning. For instance, here are two well-constructed instruments—a Peltier's electrometer and a torsion balance,—and neither of these has the slightest protection. Out of all the instruments for teaching purposes that we see in this magnificent Loan Collection, electroscopes, electrometers, and so forth, you will find not one in a hundred to have any pretence of protection whatsoever.

I have here a very simple experiment for illustrating the importance of guarding the movable parts of your electrostatic instruments from external influence. Here is a glass bell-jar, from the roof of which hangs a fine glass fibre carrying a horizontal arm of aluminium. The inside of the bell-jar is very clean, and is kept dry with the aid of a dish of strong sulphuric acid. It thus gives excellent insulation, and the aluminium needle, which was charged some hours ago, has, I have no doubt, retained its charge well. I now dry the outside of the bell-jar by passing over its surface a wire hoop, covered with cotton wick, and flaming

with spirits of wine. Now if I move this glass rod slightly electrified, or this electrified stick of sealing-wax, in the vicinity of the aluminium arm, you see at once the effect of external electrification on the movable arm, which corresponds exactly to the movable arm in the torsion balance. I will now bring the glass rod near to the outside of the bell-jar, and draw this slip of paper over the surface of the bell-jar in the vicinity of the glass rod, and then carry away the glass rod. You see that I have left the bell-jar itself with a portion of its outer surface electrified in such a way that it attracts the needle round through a right angle.

I will now wet the outside of the bell-jar with a sponge and thus make the outside of the glass a fairly good conductor. By this the needle is partly, but as you see not entirely, screened from the influence of external electrification. Yet the natural dampness of the glass cases, the dust and so forth that may be upon them, is the whole protection that the electroscopes and electrometers ordinarily constructed have against such disturbing influence.

Faraday pointed out that nothing but a complete metallic protection is sufficient. He protected the whole of the interior of the electroscopes and electrometers that he used with slips of tinfoil pasted on the inside surface of the glass cases, leaving only such spaces as were necessary for looking through. In the instruments of Sir William Thomson that I shall have to show you the movable parts are protected by brass covers, tinfoil, or by a wire cage which is often found to be sufficient and to be very convenient.

Another improvement that Faraday made with respect to the torsion balance was the introduction of what is called the heterostatic method of using the instrument. Electrometers may be distinguished into two classes, idiostatic and heterostatic. In the idiostatic class the whole electric force depends on the electrification which is itself the subject of the test. In the heterostatic class, besides the electrification to be tested, another electrification, maintained independently of it, is taken advantage of. The torsion balance as used by Coulomb was an idiostatic instrument. The electricity to be tested was divided each time between the balls

and was itself the cause of the electric force. Faraday preferred to employ the instrument thus. He gave a permanent charge to the torsion ball and placed it in a definite position, suppose 30° from the zero mark on the scale pasted round the cylindrical case of the instrument. He then introduced the carrier ball or proof plane, bearing its charge, which of course displaced the torsion ball by attraction or repulsion. Turning the button of the torsion head the torsion ball was brought back to its first position, and the torsion required in order to do this was determined.

In his celebrated *Experimental Researches on Electricity*, Faraday describes minutely the precautions that must be taken in using the torsion balance. He considered it a very valuable instrument in good hands: and in his hands it proved indeed a most valuable instrument. It was by means of it that all his grand series of electrostatic researches was carried on.

From the torsion balance of Coulomb there came a variety of other instruments more or less like it. Here for example is Peltier's electrometer, which if it had protection of the movable parts would be an excellent instrument. Instead of an arm supported on a torsion fibre we have here a long aluminium needle pivotted on a fine needle point, and to the movable needle there is attached a very short magnet. The small magnet gives the directive force instead of the torsion of the fibre. To use the instrument it is set with the magnet in the magnetic meridian and the two movable arms in contact with these two repulsion balls which take the place of the carrier ball in the torsion balance. When a charge is given to the instrument by means of this charging rod connected with the repulsion balls, the charge is also communicated to the movable needle, and repulsion is the result. The electric repulsion is balanced by the tendency of the magnet to return to its normal position in the magnetic meridian.

Here again is a similar instrument by Kohlrausch. It consists also of a conductor, a portion of which is a magnet, and repulsion plates similar to the two repulsion balls which were used in the Peltier electrometer. Kohlrausch calls this instrument a sine electrometer because in his way of using it the forces are proportional to the sines of the angles of deflection of the movable needle.

Of repulsion electrometers I have lastly to bring before you those of Sir William Thomson. The very remarkable collection of electrometers exhibited in the Loan Collection by Sir William Thomson shows the attention that he has bestowed on electrometry. Electrometers in every stage of development may be seen in his collection, but many of them you will have to go and look at in their place. It was impossible to bring them all upon the table before you; and much more was it out of the question for me to think of attempting to explain them in the two lectures devoted to this subject.

Modern electrometry is largely due to Sir William Thomson. His instruments have for the present superseded all other electrometers for practical purposes, such as the testing of telegraph cables during construction and after submersion; and I wish to call your attention to his admirable report on "Electrometers and Electrostatic Measurements," prepared for the British Association Committee on Standards of Electrical Resistance (1867), and *republished in his collected papers on Electrostatics* and Magnetism. In that report you will find full information on many points that I cannot do more than allude to, while exhibiting some of the electrometers of the Loan Collection to you.

We have three specimens of Thomson's repulsion electrometers before us. Two of them are almost identical in construction, so I have dissected one that you may see its parts.

The glass case is a thin flint glass bell, the lower half of which is coated inside and outside with tinfoil, like a Leyden jar, except that at the bottom of the inside a part of the glass is left bare. That part is filled with strong sulphuric acid, and connected by a piece of platinum foil with the tinfoil coating. The sulphuric acid performs two functions. It keeps the inside of the case of the instrument dry, preventing the deposit of moisture on the glass insulators,[1]

[1] It is curious that in almost all even of the most recent text-books on electricity we still find a distinction made between dry air and damp air as insulators. So far as we know at present, no difference whatever exists. Sir William Thomson has not been able to detect the slightest difference between dry air and damp air as to power of insulation by the most delicate experiments. The cause of the widespread fallacy on this subject is, of course, that such conductors as are used

and it also acts as a part of the inside coating of the Leyden jar, like the water in the celebrated experiment that led to the discovery of the Leyden jar.

The glass bell is enclosed in a metal case which supports it and protects it, and which is furnished with three levelling screws as feet of the instrument. The case is covered in with a circular plate of glass in a metal rim. At the centre of this glass plate is supported a torsion head, as in Coulomb's torsion balance, having an index protruding out

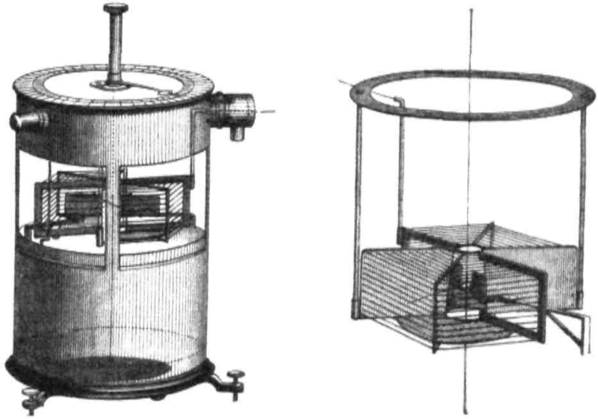

FIG. 2.

so as to move over graduations on the circular metallic ring.[1]

From the torsion head hangs a fine glass fibre, to which is attached the movable part of the electrometer. This consists of a light horizontal needle made of aluminium. To it there is attached a stiff platinum wire, hanging down

for electrical experiments are found to maintain a charge much better in dry than in damp air. The extraordinary loss observed in damp air is, however, altogether by means of the supports (of glass, shellac, &c.), the surfaces of which receive a conducting film, often invisible, of moisture from the air.

[1] The parts of the instrument were shown in the lecture. They will be easily distinguished in the accompanying diagram.

vertically and nearly reaching the bottom of the jar, and a fine platinum wire with a little weight of platinum attached to it hangs down from the end of the stiff wire and dips into the sulphuric acid at the bottom. Thus the needle is kept connected with the inside coating of the Leyden jar.

Rather less than half-way from the bottom of the jar there is a metallic ring, cemented to the inside for the purpose of supporting two *repulsion plates*. This ring and the repulsion plates are connected with the interior tinfoil coating. When, therefore, the jar is charged with electricity, repulsion takes place between the needle, also connected with the inside coating, and the repulsion plates, and the needle is driven up against two stops, connected with the plates, which limit its motion.

The ends of the needle and the repulsion plates are surrounded by a cage of fine brass wire, which is stretched on a brass framework. The framework is supported from the metallic case of the electrometer by two glass pillars. It is insulated from the case and from the needle and repulsion plates; and it has an electrode projecting outward through a hole in the outer case, by means of which the cage can be connected to any body that is to be tested.

Let me now explain the use of the instrument. The Leyden jar is charged by means of an electrode provided for the purpose, and, as I have said, the needle is repelled from the repulsion plates against the stops. The electrode of the cage is connected with the earth, or more usually with the brass case of the instrument, which is connected with the earth. The torsion head is now turned in such a way as to oppose the torsion of the wire to the electric repulsion, and the needle is forced away from the stops. The observer, looking down through the plate glass cover, now brings the needle, by means of two marks, one on the glass cover and the other on a ring below, into a "marked position," and then reads off the number of degrees of torsion on the scale over which the torsion index moves. This reading is commonly called the "earth reading," as the tested conductor—the cage—is in connection with the earth during the observation. The number of degrees of torsion required to bring the needle into the marked position depends upon the electrification of the jar; and it can readily be shown that the potential to which the jar is charged,

that is the difference of potentials between the jar and the cage or the earth, is measured by the square root of the number of degrees of torsion. When I say that the potential of the jar is measured by the square root of the number of degrees of torsion, I mean that if the jar be electrified to different potentials the square roots of the torsional readings will be in proportion to those potentials.

Now, the charge of the jar being kept unaltered, the electrode of the cage is disconnected from the earth and connected with the conductor to be tested, and the cage thus acquires the same potential as the conductor to be tested. The electrification of the cage causes an alteration in the force acting upon the needle which moves towards or from the repulsion plates, according as the electrification of the cage is similar or dissimilar to that of the jar. The torsion head is now turned so as to bring the needle back to the sighted position; and the torsion is again read off. As before, the square root of the number of degrees of torsion measures the difference of potentials between the cage, and the needle, that is also the difference of potentials between the conductor tested and the needle. The excess, positive or negative, of the square root of this last reading above the square root of the "earth reading," is the difference of potentials between the body tested and the earth; and if, as is commonly done, we regard the potential of the earth as zero, it is simply the potential of the conductor tested.

This electrometer has done good service in observations on atmospheric electricity. Indeed it was first constructed for that purpose. The electrode of the cage is connected with a collector of electricity, either with a flame-collector, as first described by Volta, or with the water dropping collector of Sir William Thomson, and earth readings and atmospheric readings are taken alternately at proper intervals. The numbers obtained for the differences of potentials are reduced to absolute measure by comparison with a few cells of a known battery; but this I will explain a little later.

[A form of portable electrometer, also due to Thomson, was next exhibited and described. The description is not, however, reproduced here, as the instrument differs little in electrical principles from that which has just been

spoken of. The suspension of the needle is different. The needle is firmly attached at right angles to a tightly-stretched platinum wire, the lower end of which is fixed, and the upper mechanically connected with, though electrically insulated from, the parts that correspond to a torsion head. See Thomson's *Electrostatics and Magnetism*, xvi., 277.]

I will conclude my lecture for to-day by showing you an electrometer, the first of a new class, and one of great interest. This is Thomson's first "divided ring" electrometer.

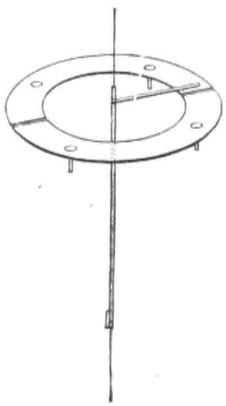

FIG. 3.

In this instrument there is a broad metallic ring cut into two parts, and each half ring is supported horizontally on two vertical pillars of thin glass rod. One half ring is kept connected with the case of the electrometer; the other can be connected by means of a proper electrode with any body to be tested. From the top of the case of the electrometer hangs a fine glass fibre which carries a light aluminium needle, projecting from a point a little above the centre of the divided ring. The needle being counterpoised projects out on one side only. It is sufficiently long to project out over the divided ring.

A stiff piece of platinum wire attached to the needle at right angles to it hangs or projects vertically down; and from the lower extremity of this a very fine piece of platinum wire hangs down and dips into the strong sulphuric acid which forms the interior coating of a Leyden jar. The case of the instrument is of glass, with slips of tinfoil pasted over it to protect the interior from external influence. There are other particulars to which I need not allude just now, as I shall have to speak of them in connection with the quadrant electrometer, a development from that which is before us at present.

Before the Leyden jar with which the needle is connected is electrified, the needle is adjusted mechanically, so that it projects over one of the divisions between the two halves of the divided ring. Now if the two halves of the ring are at the same potential, both being, let us suppose, connected with the case of the instrument, and if the parts of the instrument are quite symmetrical, then when the Leyden jar and needle are electrified, no disturbance of the needle will be experienced. But let one of the half-rings be connected with a body to be tested whose potential differs from that of the case of the instrument to which the other half-ring is connected, and there will plainly be attraction or repulsion of the needle, according to the nature of the electrification of the tested body, and the needle will move towards one half-ring or towards the other.

Now let me in conclusion refer to one very remarkable application that Sir William Thomson has made of this electrometer. It was chiefly for that purpose that I have brought it before you, because the instrument itself, though in earlier times it did admirable service as an electrometer, has now been superseded by the quadrant electrometer. By means of this instrument Sir William Thomson was able to furnish a test between Volta's contact theory of his pile and the rival chemical theory. The fundamental statement of Volta's theory was that a piece of metallic zinc and a piece of metallic copper put in contact assume different potentials, the zinc becoming positive with respect to the copper. The supporters of the chemical theory denied this, and explained away the experiments that were adduced in support of it. It is now, however, regarded as established, and here is one of the experiments of Thomson

to prove its truth. Taking away the two half-rings of brass he substituted this ring which I now show you, one half of which is of copper and the other half zinc, the two soldered into one ring. The needle and Leyden jar of the electrometer being completely discharged, the needle was mechanically adjusted so as to hang with its extremity over one of the soldered joints between zinc and copper. The needle and jar were then electrified, first positively and then negatively, and it was found that, when positive the needle moved from the zinc half-ring towards the copper half-ring, and when electrified negatively it moved from the copper half-ring towards the zinc half-ring. Thus the zinc and copper half-rings in contact behaved as would two insulated half-rings of the same metal, one of which, corresponding to the zinc, is electrified positively relatively to the other.

ELECTROMETERS.

LECTURE II.

THE instrument to which I will now direct your attention is Thomson's quadrant electrometer. It is the most recent and most complete development from the divided ring electrometer which we examined yesterday. The semicircles of the divided ring over which the movable needle swings are in the quadrant electrometer replaced by four quadrants of a hollow cylindrical box, within which is the movable needle; and the needle, instead of projecting on one side only of the axis of suspension, as in the divided ring electrometer, is symmetrical about that axis.

In the quadrant electrometer we have first a white flint glass bell-jar, surrounded and supported, mouth up, by a metal casing. The outside is partially coated with tin-foil; the inside contains strong sulphuric acid a couple of inches deep. This arrangement gives us, as we have already seen in former cases, a Leyden jar; and with the inside coating of the Leyden jar the movable needle is connected.

Secondly, we have the "main cover" of the instrument, a circular brass plate, covering the mouth of the jar and screwed down to the metal casing of the jar. Over a large circular hole in the main cover stands what is called the "lantern." The lantern is of brass, but with a window in front. It carries the "gauge," and three electrodes project from the top of it. [See Fig. 4.]

The four quadrants (Fig. 5) are supported each on a short pillar of glass projecting downwards from the main cover of the electrometer. The supports of these glass pillars are movable in radial slots, and the quadrants can be drawn out from or pushed in towards the axis of suspension. When the instrument is in adjustment they are pushed in

as near as possible without touching, and thus form parts of a cylinder, divided only from each other as it were by two saw-cuts. Thus also they are arranged symmetrically around the needle. One of the four is capable of adjustment by a screw turned by a milled bead. Slight motions of this quadrant serve for adjusting the zero of the instrument occasionally.

The quadrants are connected in pairs by fine wires, the *opposite* quadrants being connected together ; and from one

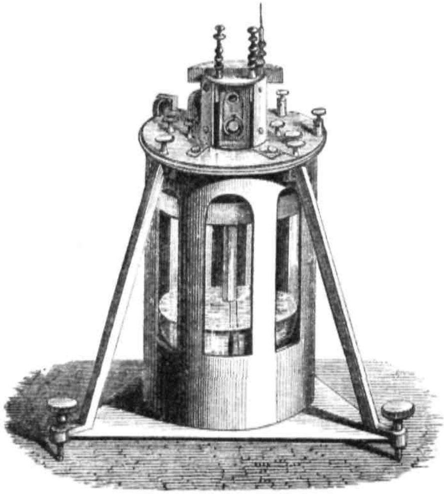

Fig. 4.

quadrant of each pair rises an electrode which passes insutated through the top of the lantern. These electrodes serve to connect two conductors, whose difference of potentials is to be measured, one with each pair of quadrants. The third electrode (see Fig. 4), passing up through the top of the lantern, is for charging and discharging the Leyden jar of the electrometer.

We next come to the "needle." It is a thin flat piece of sheet aluminium, shaped perhaps more like the paddle

of a canoe than like a needle (Fig. 5); but, you know, in electricity and magnetism the name needle is commonly applied to bodies that in shape and size are very unlike that from which the name is taken. The needle is borne horizontally on a stiff vertical platinum wire, which passes through its centre upward and downward. The stiff platinum wire is attached to a small cross-bar, which is carried by a bifilar suspension. [See Fig. 6, where the upper end of the stiff platinum wire is seen coming up through the "guard tube" and having the "mirror" attached. The bifilar fibres of the suspension are seen coming up to two pins, *c* and *d*.] To the lower end of the stiff platinum wire a very fine platinum wire is attached. This carries a small platinum weight, which hangs down and dips into the sulphuric

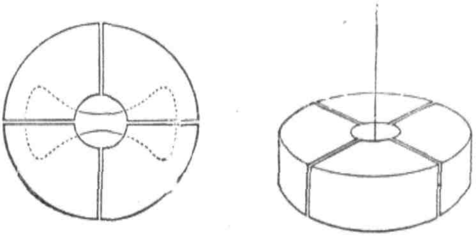

FIG. 5.

acid at the bottom of the jar; and the needle is by this means kept in connection with the interior coating of the charged Leyden jar.

I have got here the suspension plate of an electrometer to show you (Fig. 6). It is supported and insulated by a glass pillar, which rises from the main cover, and it is covered by the lantern. On the face of it are five pins, (*a*) (*b*) (*c*) (*d*) and (*h*), which can be turned by means of a square pointed key. The threads of the bifilar are wound up on the pins (*c*) (*d*), and (*c*) and (*d*) are capable of being turned so as to adjust the lengths of the fibres to be equal. The pins (*c*) and (*d*) are carried on springs (*e*) and (*f*), which are screwed to the face of the suspension plate by screws shown about one-third of way from the bottom of that plate. The pin marked (*h*) is a conical plug, which passes in between

the springs and screws into the plate behind, and by turning it in or out the conical plug presses the springs (*e*) and (*f*) apart, or allows them to approach. Thus the upper points of the bifilar suspension are separated or brought nearer together, and the sensibility of the instrument is diminished or increased. The pins (*a*) (*b*) are screwed into the springs (*e*) (*f*), and press against the plate behind.

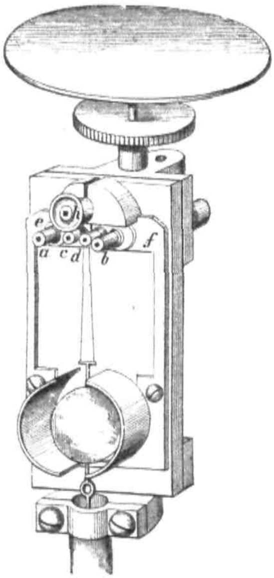

FIG. 6.

When one or other of them is turned it brings forward the neighbouring point of suspension, or allows it to recede.

Lastly, as to this part, the platinum wire carrying the needle has a small circular mirror attached to it (see Fig. 6), and all the movable parts are carefully guarded from external influence;—the platinum wire, both above and below the needle, by a metal "guard tube" through which it passes, and the mirror by a little cylindrical hood projecting some distance from the suspension plate.

The mirror is a small concave mirror of silvered glass. It is about the size of a threepenny-piece. It is extremely light, weighing only about a third of a grain. To make these little glass concave mirrors (which are also used for reflecting galvanometers, where extreme lightness is even more essential than in the present case) was a matter of considerable difficulty. The following plan is now adopted. A large number of little circles of the finest microscope glass are silvered. When they are to be used for reflecting galvanometers, four minute magnets are cemented to the back. The mirrors are then tested, to see that each gives a good image of a lamp reflected from it; and, out of perhaps fifty tried, ten or fifteen may be found satisfactory. This plan of selection by trial gives mirrors that afford a perfect image, and which are lighter than any that can be obtained by the plan of grinding at first adopted. The concavity of the mirrors is such that the rays of a lamp placed about one metre ($39\frac{1}{3}$ inches) from the mirror, and reflected from it, are brought to a focus on a screen at the same distance from the mirror.

In the use of the electrometer a lamp is placed in front of the mirror and its rays, passing through a narrow vertical slit, fall on the mirror and are reflected from it. The reflected *rays are brought to a focus on a long horizontal screen at* right angles to the line from the slit to the mirror, and just above the slit. The screen, which has marked on it a finely-divided scale, is one metre distant from the mirror.

Now you will see that the position of the image of the slit on the scale depends upon the angle at which the rays proceeding from the slit to the mirror fall upon the mirror; and as the mirror turns with the needle, the position of the image on the scale depends upon the position of the needle. When the needle is at its position of zero deflection, the image of the slit is seen at a point on the scale just above the slit. When the needle is deflected to one side or other, the image on the scale is also deflected, and by a well-known optical principle the angle of deflection of the reflected rays is twice the angle of deflection of the mirror,—that is of the needle.

We are now prepared to understand the principle on which the use of the quadrant electrometer depends. We have in this case three bodies, two of them fixed (the two pairs

of quadrants), and one (the needle) movable about an axis, the arrangements being of the symmetrical kind that I have described, and the movable body is maintained at a constant high potential. The potentials of the two fixed conductors are different, and it is this difference which we wish to measure. Let V_0 denote the potential of the needle, let V_1 and V_2 be the potentials of the two fixed conductors; let θ be the angle of deflection of the needle from the position which it would occupy were V_1 and V_2 equal, then it can be shown that if V_1 be greater than V_2,

$$\theta = C(V_1 - V_2)[V_0 - \tfrac{1}{2}(V_1 + V_2)] \quad . \quad . \quad (A)$$

where C is a constant. Now if V_0 be very great in comparison with V_1 and V_2, the second term, $\frac{1}{2}(V_1 + V_2)$, of the last factor may be omitted in this expression, and we obtain

$$\theta = C(V_1 - V_2)V_0. \quad . \quad . \quad . \quad . \quad (B)$$

In the use of the quadrant electrometer this is practically the state of affairs; for, as I have just said, V_0 is the potential of the Leyden jar with which the needle of the electrometer is connected, and the potential of the jar is always maintained very great indeed in comparison with that of any electrification that this delicate instrument is employed to measure.

Taking now the formula (B) we see in the first place that θ, the angle of deflection, is in simple proportion to the difference of potentials $V_1 - V_2$ of the quadrants, that is, of the two bodies tested, V_0 being kept constant. Secondly, we see that as θ is proportional to V_0, $V_1 - V_2$ being constant, the deflection for any given difference of potentials is greater the greater V_0. Thus the sensibility of the instrument is proportional to the potential of the needle. By altering the charge of the Leyden jar, therefore, we may alter the sensibility of the electrometer.

To keep the charge of the Leyden jar constant, which you see is essential in carrying out any set of measurements which are to be immediately comparable, there is a "gauge" connected with the electrometer, as well as a minute electric machine within the glass jar, and supported from the main cover, by means of which the charge can be increased or diminished. The gauge is really an electrometer, precisely similar in construction to one that I shall

have to describe immediately, the portable electrometer, but turned upside down, and used idiostatically. Above this horizontal plate (the circular plate at the top of Fig. 6), connected with the suspension plate of the needle, there is an arrangement resembling somewhat a cart-weighing machine. G is a brass plate, with a square hole cut in it. The hole is over the middle of the circular plate, connected with the suspension plate. *p* is a square of light sheet aluminium; it is connected with the long arm *h*, the square *p* and the long arm *h* being in one piece. This lever is carried on a tightly-stretched platinum wire *f*, round which as a fulcrum it is perfectly free to move; and torsion is given to the wire *f*, so as to tend to raise *p* upwards and to depress the arm *h*. But when the little square *p* is attracted by the plate connected with the needle and jar, the

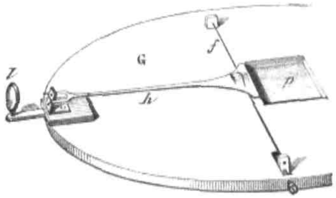

FIG. 7.

attraction draws *p* downward against the torsion of the wire *f* to a fixed position.

The extremity of *h* is cut out into a little fork, across which a fine black hair is stretched, and behind the hair there stands a minute white porcelain plate with two round black dots upon it. The arrangement is looked at by the magnifying lens *l*, and the plate *p* is in the proper position when the hair is midway between the two dots. The Leyden jar of the electrometer is then charged up until this is the case.

The little electric machine for increasing or diminishing the charge of the jar is one of a kind first invented by Nicholson, known by the name of Nicholson's Revolving Doubler, and used by Volta in some of his researches. Recently several such machines have been constructed. That of Holtz, now well known, makes use of the "com-

pound interest principle," involved in the action of the Doubler.

I have just shown you that the deflections observed by the quadrant electrometer are in simple proportion to differences of potentials between the quadrants, or differences of potentials between any two conductors tested. From these scale readings we must deduce numbers giving the differences of potentials in absolute measure. One way of doing this is to compare the indications of the quadrant electrometer with those of an absolute electrometer, such as I am about to describe immediately, and to deduce "the constant" for the quadrant electrometer. The scale readings may then be reduced to absolute measure by multiplying by the constant; and the constant of the instrument is the same so long as the gauge is kept in the same condition, and the quadrants in the same position as when the comparison with the absolute electrometer was made. Another method, which can at any time be applied with the greatest ease, is to find the deflection given by a galvanic cell of known electromotive force, and deduce the constant in that way. For example, let a Daniell's cell be applied to the quadrant electrometer, and let us suppose that the deflection is seventy-five scale divisions on one side of the middle position, and when the electrodes of the cell applied to the electrodes of the electrometer are reversed, a deflection of seventy-five divisions on the other side of the middle is observed. Now the electromotive force of a Daniell's cell, or the difference of potentials producible by a Daniell's cell, is well known from the experiments of Sir William Thomson.[1] It is 1·12 of the unit termed by practical electricians a *volt*.[2] Hence, dividing $\frac{75}{1 \cdot 12}$, we get 67 as the deflection produced on the electrometer by a difference of potentials of one volt. A double deflection would be produced by a difference of potentials equal to two volts, and so on.

[Some of the applications of the quadrant electrometer were next very briefly referred to, particularly its application to observation of atmospheric electricity.] For this purpose it is admirably suited. The mirror electrometer is the

[1] *Proc. Roy. Soc.* 1860, and *Reprinted Papers*, xviii.

[2] One volt is equal to 10^8 centimetre-gramme-second electro-magnetic units. See Everett's *Illustrations of the Centimetre Gramme-Second System of Units*, or F. Jenkin's *Electricity and Magnetism*.

only form which can be used for obtaining with the aid of photography a continuous record. At Kew Observatory a quadrant electrometer is now in action; and it is very much to be desired that at every meteorological observatory atmospheric electricity should be made a subject of continuous observation. It seems strange that it has not already received more attention; or rather that but few observatories have hitherto given the subject any attention whatever. [Some of the photographic traces obtained at Kew with an older form of reflecting divided ring electrometer, exhibited in the Loan Collection, were shown and described.]

I have just one other class of electrometers to refer to. Electrometers of this class are called "attracted-disc electrometers." The first instruments of this kind were made by Sir W. Snow Harris, but they were in many ways very imperfect, and, as I had occasion to remark yesterday, his interpretation of the results which he obtained led him into the error of disputing the truth of the laws of Coulomb.

[The diagram (Fig. 8) shows an electrometer of Snow Harris. As exhibited, it is connected with a Leyden jar, J, which is to be tested, while the Leyden jar is being charged from an electric machine through the well-known "unit jar" L.] In this electrometer the attraction between two plates *a* and *d*, one of which, *a*, is insulated and electrified, is determined by weighing, as in a common balance. The plate *a*, being electrified, attracts *d*, and the attraction is counterbalanced and weighed by putting weights into the pan P of the balance. Now if we have two circular electrified plates attracting each other, the "lines of force" between the plates being straight lines, and if we measure the areas of the plates, the distance between them and the force of attraction, it is easy to deduce the difference of potentials between them in absolute measure. This was practically what Snow Harris attempted.

As I have said, however, his arrangement was very imperfect. The movable plate *d* is in no way protected from inductive influence, and of the nature or extent of that influence there is no possibility of taking any account. Moreover, the arrangement of the instrument is not such as to enable us to consider the lines of force between the attracting plates to be sufficiently nearly straight lines. Sir William Thomson, in taking advantage, for an absolute

electrometer, of the principle of weighing the attraction between two discs, has remedied both these defects. The absolute electrometers are not very portable, and thus there is not one to be seen in the Loan Collection. These diagrams will, however, I hope, enable you to understand the principles of the instrument.

If we have two electrified discs attracting each other, their

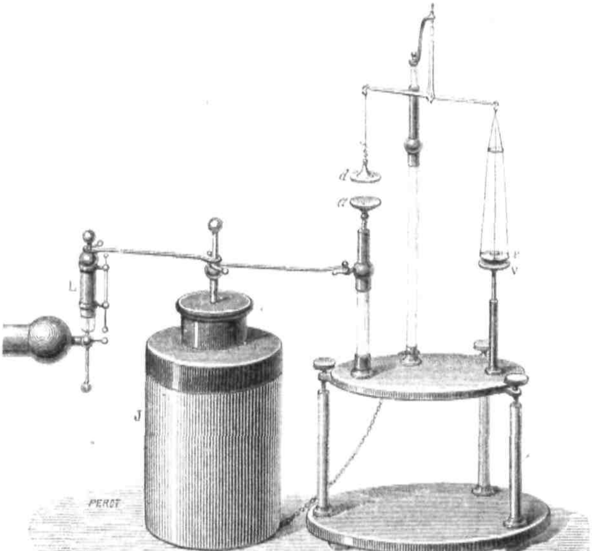

Fig. 8.

radii being two or three times as great as the distance between them, it can easily be shown that there is a region about the middle where the lines of force are sensibly straight lines. And this condition will also be practically realised by such an arrangement as is shown in Fig. 9. A and B are two insulated electrified plates. In the middle of B a circular hole is cut, which is filled up by a very light movable disc of aluminium, C, borne at the end of the lever L.

The disc C, when "in position," is placed so that its lower face is as nearly as possible in the same plane with the lower surface of the "guard-ring" B. The interstice between B and C is extremely narrow in comparison with the diameter of C and with the distance between the plates. This is the electrical part of the absolute electrometer.

The lever L is pivotted on a torsion wire, which is stretched between two insulated metallic pillars PP. Q is a counterpoise. At the end of the lever L there is an index hair; and a lens *l* is placed so as to view the position of the hair *relatively to two black dots on an index plate.* When the hair is between the two black dots the plate C is *in position,*

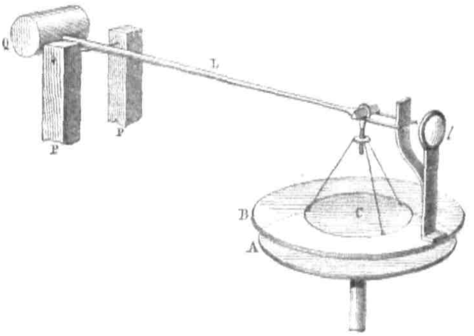

FIG. 9.

with its lower surface in the plane with the lower surface of the guard-ring B. The guard-ring B is kept metallically connected with the metal pillars PP, and thus with the lever L and with the plate C, which, however, is not in direct contact with B, but is perfectly free to move. The lever L is hung so that when A and C are at the same potential the counterpoise preponderates, and the hair at the extremity of L rises above the sighted position; and before each series of experiments with this electrometer the force required to bring the hair to the sighted position is determined by placing a small weight on C and a rider on the arm L. When this has been done the weights are removed. But when A and C are at different potentials the attraction

between them draws the plate C down, and the force with which C is attracted downward depends on the difference of potentials between the plates A and C, and on the distance between them. The plate A can be raised or lowered by means of a screw, and, when an experiment is being made, its distance from C is adjusted till the index hair is in the sighted position. When that is the case it is known that the attraction between the plates is equal to the force of gravity on the weight previously determined.

Now it was shown by Sir William Thomson in one of his earliest papers on electricity, a paper in which he considered the validity of the objections raised by Snow Harris against the laws of Coulomb, that in a case such as we have been considering, where the lines of electric force are straight lines between two attracting discs, the force, F, of attraction, will be given by the formula

$$F = \frac{V^2 S}{8 \pi D^2}$$

in which V is the difference of potentials of the two plates, S the surface of the smaller, and D the distance between the two. From this we obtain at once

$$V = D \sqrt{\frac{8 \pi F}{S}}.$$

Hence if we measure the area S, the distance D, and the force F, which, as I have said, is done by the previous weighing, we can at once calculate V; and it is to be noticed that if F is expressed in absolute units of force, the numerical value of V deduced by this formula will be obtained in absolute electrostatic units of potential.

In working practically with the absolute electrometer, however, Sir William Thomson found great difficulty in measuring D, the distance between the plates, with sufficient accuracy. He therefore adopted a mode of employing the instrument somewhat different from that which I have indicated. The plate A is connected with a separate electrometer, and with a Leyden jar and replenisher, and is maintained at a constant high potential. To measure the potential of any conductor, the plate C is first connected with the earth, the plate A is adjusted till the hair is in the sighted position, and a reading taken, called the "earth

reading." Let D be the distance of A from C for this adjustment. The conductor to be tested is then connected with C, and by moving the plate A the hair is again brought to the sighted position. Let D′ be the new distance. If V′

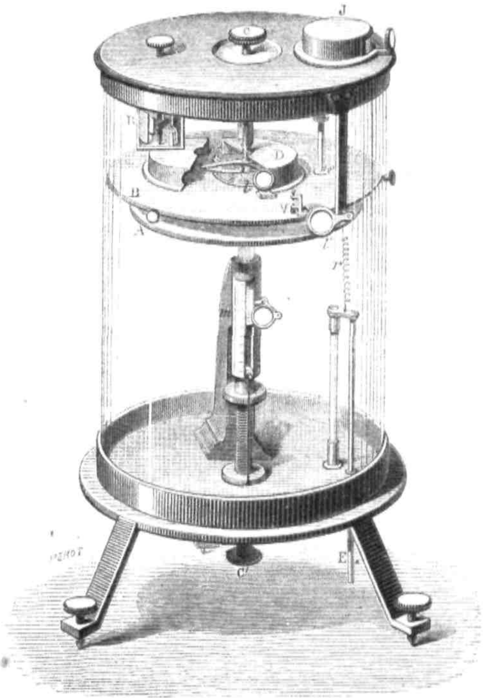

FIG. 10.

and V be the potentials of the earth and of the tested conductor, we see at once that

$$V' - V = (D' - D) \sqrt{\frac{8 \pi F}{S}};$$

or if V, the potential of the earth be taken as zero, which

is usually done, we have the potential of the conductor tested

$$V' = (D' - D)\sqrt{\frac{8\pi F}{S}}$$

Now it is plainly unnecessary to determine the absolute distances D′ and D. The difference, or the change of distance from a given position of the lower plate A, is all that is required. This Sir William Thomson has found it easy to measure with sufficient exactness by means of a micrometer screw.

The form of absolute electrometer that I have now described has been recently superseded by the instrument shown (Fig. 10). The principles of the instrument are precisely the same as those that I have been explaining. The plates A and B, Fig. 10, correspond to those marked A and B, Fig. 9. The plate C of the instrument just described is, in the new instrument, hung somewhat differently, being suspended on three springs somewhat like coach springs. The movable plate and its suspending springs are covered and protected from inductive influence under a cylindrical cover D, of which a part is shown displaced in the diagram. The plate A is moved up or down by a micrometer screw moved by a milled head C′ below. The upper part of the glass cover is a Leyden jar, with which is connected a gauge J, similar to that of the quadrant electrometer, and R is a replenisher for maintaining the Leyden jar at a constant potential. In this instrument, however, it is the guard-ring B and the suspended disc that are maintained at a constant potential; while the conductor to be tested is connected with the electrode E, which is connected with the plate A by the spiral spring R. There are other particulars of which I should like to speak to you, had we the instrument itself before us, and did our time, which I perceive is almost gone, permit.

Lastly, I must show you a very beautiful instrument, the Portable Electrometer (Fig. 11). A few words will suffice for its description. In the bottom of a cylindrical glass jar, of which a considerable part is coated with tin-foil, so as to form a Leyden jar, there is fastened a circular brass plate, with a movable aluminium square, and long index arm, precisely the same as the gauge of the quadrant electrometer

(Fig. 7), only that it is inverted. This plate is kept electrically connected with the interior coating of the jar; and the jar is kept at a high potential. There is a horizontal brass plate A, which is borne on a glass column at its middle, and insulated from everything but the spiral spring *r*. This spiral spring is connected to an electrode which passes, insulated, out through the top of the case of the instrument, under the "umbrella" D; and by means of this electrode the plate A may be connected with any conductor whose potential it is desired to measure. The plate A can

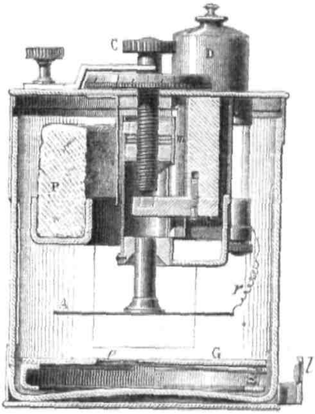

FIG. 11.

be raised and lowered. For this purpose the glass column by which it is suspended is attached to a hollow nut *m*, which works up and down on a vertical micrometer screw turned by the milled head C. A vertical scale [not shown in the diagram] and the horizontal graduated circle connected with C, serve to read off changes of distance of A relatively to the attracting plate G; P is a mass of pumice, on which drops of the strongest sulphuric acid are poured, for the purpose of keeping the interior of the case dry.

The portable electrometer is used in a way very similar to that in which the absolute electrometer is used. The

electrode connected with the plate A is first connected with the outside casing of the instrument, that is practically with the earth, and the micrometer screw is turned till the index hair is in the sighted position. The earth reading is then taken. Next the electrode is connected with the conductor to be tested. If the conductor is at a different potential from the earth, the attraction of the plate A for the plate G is altered. The micrometer screw is again turned till the index takes the sighted position. From the difference of the two readings the potential of the conductor tested may be deduced. This instrument, however, requires a preliminary determination of constants in a way similar to that which I have already indicated for the quadrant electrometer, in order to enable us to reduce the numbers deduced from the scale-readings to absolute measure.

The portable electrometer is admirably fitted for observation of atmospheric electricity. As I show you it now it is ready for that purpose. To the electrode of the plate A a long stiff wire is connected, which bears at its point a slow-burning match of blotting-paper, soaked in nitrate of lead, and then dried. When I light the match the heated particles rushing off from it quickly bring the potential of the plate A to be the same as that of the atmosphere just at the point where they are rushing off. By turning the micrometer screw I quickly adjust the position of the plate A till the index is in the sighted position.

ON THE APPARATUS RELATING TO VEGETABLE PHYSIOLOGY.

BY SYDNEY H. VINES, B.A., B.SC.; FELLOW OF CHRIST'S COLLEGE, CAMBRIDGE.

In describing and explaining to you the construction and the use of the instruments relating to Vegetable Physiology *which have been contributed to the Loan* Exhibition, I will follow the order in which they are arranged in the Catalogue.

The first object to which I would draw your attention is this sketch (No. 3904 in Catalogue) exhibited by Dr. Velten, Physiologist to the Institute for experiments relating to Forestry in Vienna—of an apparatus devised for the investigation of the influence of temperature upon living organisms. It consists of a box of zinc with double walls, in the roof and sides of which panes of glass are fixed, by means of which all that goes on within can be observed, and in the sides are openings through which the hands—cased in india-rubber gloves, to prevent any sudden change of temperature—may be introduced into the interior. The space between the walls is filled with a liquid (water or oil), the temperature of which can be raised by applying heat below, or lowered by placing a refrigerator in the wooden case made to receive it.

The use of this apparatus will, I think, be most clearly demonstrated by a description of some experiments which might be performed with it. In performing such experiments we must be most careful to arrange that the phenomena which we are to observe shall be dependent only upon that one agent the action of which we are investigating, namely, temperature. We must therefore

prevent as far as possible the action of other agents upon the organism under experiment, and this may be done either by entirely removing the agents in question, or by rendering this action constant throughout the experiment. In the present case the disturbing agents are light and moisture. The action of the former may be prevented by keeping the apparatus in the dark, and of the latter, by *maintaining the atmosphere within the case at its point of saturation.*

The first and simplest experiment which we might perform would be one in which a plant should be exposed to a constantly increasing temperature, and we should find that when a certain degree in temperature was reached, the plant would die. In the next place, we should find that, if a plant were exposed to a constantly decreasing temperature, after a time death would ensue. From these experiments we should learn that plant-life can be maintained only within certain limits of temperature—and by repetitions of these experiments we should be able to determine these limits with accuracy. Roughly speaking, they are 0° C. (32° F.) and 50° C, (122° F.) (Sachs).

The next step would be to investigate the relation existing between each one of the phenomena of plant-life and the temperature of the surrounding medium. Suppose, for instance, that we are investigating the effect of temperature on growth. Taking the Pea as the subject of experiment, we should find that it would not grow at all at a temperature lower than 6° C., that the rapidity of its growth would increase with every rise of temperature up to 26° C. —that any further rise of temperature would be attended with diminished rapidity of growth, and that, at a temperature of about 40° C., growth would be entirely arrested (Koppen).

A series of such experiments upon any plant would show that all its functions are affected by temperature in the manner just described with reference to growth. We should find that, for each function, there were certain definite limits of temperature within which that function could be performed, and that between these limits there was a degree of temperature which corresponded to the maximum activity of the function in question.

The next apparatus in the catalogue (No. 3935) is but a

special application of the preceding to the investigation of the relation existing between the process of germination and temperature, devised by Prof. Cohn, Director of the Institute of Vegetable Physiology in the University of Breslau. It consists of a chamber with double walls made of tin—the space between them being nearly filled with water—within which are several tin trays on each of which are several small dishes of porous earthenware. The seeds, after having been soaked for twenty-four hours in water, are placed in the earthenware dishes, water being poured into the tin trays in order to maintain the seeds in a sufficiently moist state. Heat is then applied to the chamber by means of a small regulated gas flame, and the temperature is raised to a degree which previous experiments have shown to be the most favourable to the process of germination.

The use of an apparatus of this kind to the physiologist is obvious. By its assistance he is enabled to control all the conditions upon which the process of germination depends, and he can ensure a supply of material at any time for the investigation of this process either from a physiological or from a morphological point of view. But its usefulness is by no means limited to the study of the process of germination as it occurs in the seeds of the higher plants. It is extremely useful for the purpose of cultivating various kinds of fungi, since it affords all the conditions necessary for their growth, and by this means much of the life-history of these fungi may be brought under observation.

Let us consider for a moment what lessons in Vegetable Physiology this apparatus teaches. To the seeds germinating, or to the moulds growing within it, it affords heat, air, and moisture, whilst it shuts out the light from them. Hence we may infer that heat, air, and moisture are essential to the germination of seeds, and to the growth of moulds, whereas the presence of light is unnecessary.

With this apparatus we can further ascertain what degree of temperature is most favourable to the germination of a seed or to the growth of a fungus, and between what limits of temperature these processes will occur. For instance, it has been found that wheat or barley will germinate at a temperature so low as 5° C., and the highest

temperature at which the seeds of these plants have been observed to germinate is 38° C.—the temperature at which germination is most active being from 20° to 25° C. Experiments of this kind performed upon the seeds of a variety of plants show that the range of temperature through which germination may take place, extends from 0° to 50° C.

The next piece of apparatus (No. 3936) is quite classical in the history of Vegetable Physiology. It is constructed and exhibited, like the preceding, by Prof. Dr. F. Cohn, of the University of Breslau, and is a modification of the original apparatus invented by Knight in the year 1806. It consists of a large tin box in which is an axle bearing a water-wheel at one end and a disc of cork at the other. A stream of water is introduced through the roof of the box which sets the water-wheel in motion, causing, at the same time, rotation of the disc of cork, into which are fixed several pins, each of which transfixes the cotyledons of a pea. This apparatus has been in action for the last few days, and as you see, the peas have germinated. I would ask you particularly to notice that in each case the young stem has grown inwards towards the centre of the axle, whereas the young root has grown outwards in the opposite direction.

Knight was led to invent this apparatus from a consideration of the fact that the young root of a germinating seed always tends to grow towards the centre of the earth, whatever the position of the seed may be, whereas the young stem always grows in the opposite direction. Duhamel had already shown that, if the position of a seed be reversed several times whilst it is germinating, the young root and stem soon resume the original direction of their growth after each change of position. These phenomena were ascribed at that time to the action of gravity upon the growing tissues, but these views were purely hypothetical, resting upon no experimental basis: it, remained for Knight to provide the necessary evidence.

By means of this apparatus, the stem and root of the young plant are exposed to precisely similar conditions of light, moisture, air, and temperature, the action of gravity is eliminated, and the action of a considerable centrifugal force is introduced. Under these circumstances we must

admit that it is under the influence of this force that the root grows centrifugally, and the stem centripetally—that is to say, that the direction of growth of each of these organs depends upon a relation existing between a purely physical force and their growing cells.

Applying these results to the ordinary downward growth of roots and upward growth of stems, we are at once led to the conclusion that these organs assume their respective directions of growth in consequence of the action of gravity upon their growing cells.

The mode in which gravity acts upon growing cells may be to some extent rendered intelligible by means of a diagram, (Fig. 1). Suppose A B to be a young plant of

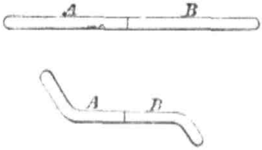

FIG. 1.

which A represents the stem and B the root, each consisting of a single cell. At first the position of both is horizontal, but as growth proceeds A turns upwards and B downwards. This change in direction is due, in the case of A, to the fact that the lower portion of the cell-wall has grown more rapidly than the upper portion, and in the case of B, to the fact that the upper portion of the cell-wall has grown more rapidly than the lower.

Why the growing cells of young roots should be so affected by physical forces, such as gravity and centrifugal force, that the direction of their growth should follow that of the action of these forces, and why the growing cells of young stems should be affected in a precisely opposite manner, are questions to which no satisfactory reply can be given at present.

To this behaviour of the growing cells of plants to the action of gravity the name *geotropism* has been given—those parts which obey it in the direction of their growth being termed positively, those which oppose it negatively,

has been made to pass through the electro-magnet B, and it has attracted the steel rod bearing the pencil. You will also observe that the pencil remains in this new position

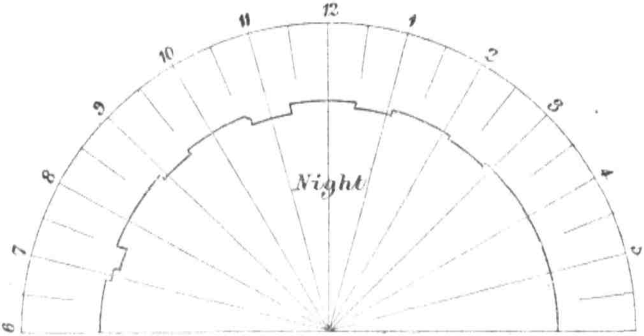

FIG. 3.

for some time—in fact, for just so long a time as the pressure of the toothed wheel keeps the spring in contact with the binding screw *b*, for when this pressure is removed the pencil resumes its original position. We see then that each of these depressions which we observe in the tracing corresponds to the time which is required for the rotation of a single tooth of the wheel.

This is made more evident when the tracing on the disc is tabulated in the manner illustrated by Fig. 4. In this the Arabic figures indicate hours, the Roman figures the depressions in the disc-tracing, or, in other words, divisions of the toothed wheel.

Fig. 5 is the tracing, similarly tabulated, taken during the twelve hours between 6 A.M. and 6 P.M.

Fig. 6 is a tabulated arrangement of the two preceding figures, by means of which the curve of the velocity of growth during the twenty-four hours is obtained. In this the smallest velocity is taken as the unit of measurement for the ordinates.

So much then for the mechanism of this apparatus, and for the mode of using it. Let us now go on to consider

what we are to learn from the tracings which we have obtained from it. From an inspection of them we find that growth took place between six o'clock in the evening

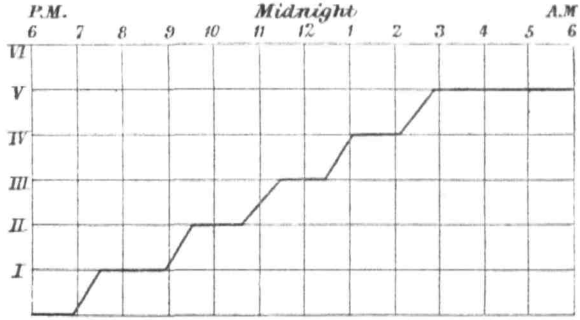

FIG. 4.

and ten o'clock the following morning, after which a period of inactivity followed, until, towards four o'clock in the

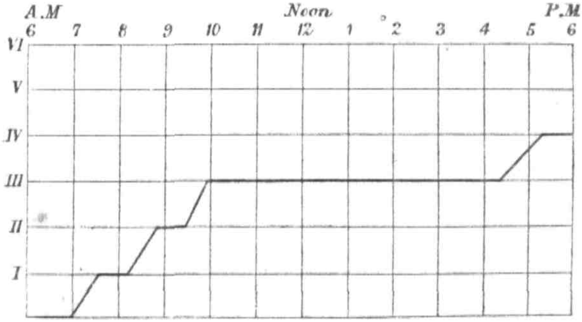

FIG. 5.

afternoon, the process of growth recommenced; and we find further that the period of growth is divided into two well-marked epochs, the one extending from 7 P.M. until

3 A.M., the other from 7 A.M. to 10 A.M. We have now to account for these intermissions in the process of growth, and to discover whether they bear any relation to variations occurring in the external circumstances to which the plant was exposed. These external circumstances were (1) a supply of air, (2) a supply of moisture, (3) the action of temperature, (4) the action of light.

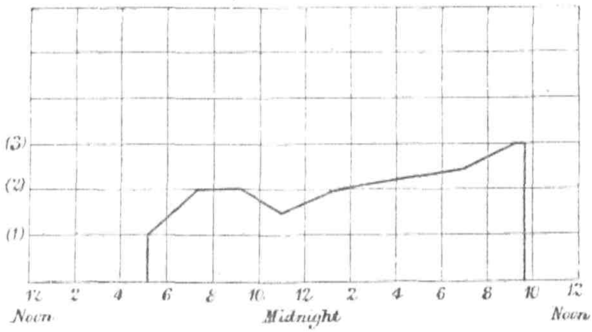

FIG. 6.

We may fairly assume that of these the first two were invariable during the course of the experiment, and we need not therefore regard them any longer as elements of the problem. It remains for us to consider the action of temperature and of light upon the growth of the plant.

In discussing the question as to how much changes of temperature have contributed to the production of these tracings, we must recall the general law at which we have already arrived, which indicates the relation existing between temperature and plant life. From this we are able to assert that a slight rise of the atmospheric temperature will stimulate the process of growth, whereas a slight fall will depress it. Now we shall be probably correct in saying that the temperature of the air will gradually sink from 6 P.M. until sunrise (3 A.M.), after which it will gradually rise, until it begins to sink again in the afternoon (4 P.M.). We should therefore expect to find that the process of growth would be arrested, or at

any rate would proceed but slowly, from 6 P.M. to 3 A.M., and that then it would go on with increasing rapidity until the afternoon. We find, however, as a matter of fact, that growth has gone on vigorously from 6 P.M. to 3 A.M., and that the tracings support our supposition only in regard to the growth which took place between 7 A.M. and 10 A.M. We are forced to the conclusion that the growth of the plant has been influenced by some force having an effect upon it contrary to that of temperature—and we must seek this force in the action of light.

Let us employ a process of reasoning in the investigation of this question, similar to that which we employed in the case of temperature. We may begin by assuming that which already appears to be probable, namely, that the effect of light is to retard growth, and we may go on to see whether or not this hypothesis satisfactorily explains the phenomena. We should expect to find that with dim light (from 6 P.M. to 3 A.M.) the process of growth would commence, or, if already in progress, would go on with increased rapidity; and we should further expect to find that, after sunrise, the rapidity of growth would diminish until it was completely arrested. The tracings bear out the first of our expectations with precision, but the epoch of growth extending from 7 A.M. to 10 A.M. is quite contrary to the second. It is evident from these facts that the intermissions of growth are not due to the action of light alone, nor to the action of temperature alone: it remains for us to see whether they are not due to the combined action of these influences. From 7 P.M. to 3 A.M. we have a dim light and a decreasing temperature. Of these conditions, the former is favourable, the latter unfavourable, to the process of growth. If we reflect that the diminution of temperature must be after all comparatively small, we may fairly conclude that the absence of light has had a greater effect than the diminution of temperature, and that therefore the process of growth has on the whole been favoured. From 3 A.M. to 7 A.M. we have a rapidly increasing intensity of light and a slowly rising temperature, and we find, as we might have expected, that the tracing indicates a period of inactivity. From 7 A.M, to 10 A.M. we have again an increasing intensity of light and a rising temperature, the latter taking place more rapidly than the former,

where the tracing indicates a short epoch of growth. From 10 A.M. to 4 P.M. the influence of a high temperature seems to be entirely overcome by the action of an intense light, so that it is not until the afternoon, when the light begins to wane, that the growth is resumed.

Hitherto we have regarded only Figs. 4 and 5, which indicate the actual absolute growth of the plant. Let us briefly consider Fig. 6, which shows the relative velocity of growth at different times. The growth was slowest at 4 P.M.—at 8 P.M. the figure indicates that its rapidity was twice as great—then we observe a slight diminution of velocity towards midnight, but an increase soon followed and continued until 10 A.M., when the greatest velocity—three times that at 4 P.M.—was attained.

From the discussion of an experiment performed with this apparatus we have derived a confirmation of the general law which expresses the relations between plant-life and temperature, and we have learned that light has a retarding effect upon the process of growth. It may be objected, however, that the intermissions of growth recorded in the tracings may not, after all, be entirely due to the influence of external conditions upon the plant, but that they may perhaps be referred to some inherent property of the growing tissues by virtue of which a certain periodicity in the process is produced. Experiments have been instituted in which the external conditions were prevented, as far as possible, from undergoing any variation during their performance, and in these no such intermissions of growth were observed as are indicated in Figs. 4 and 5.

We may now go on to inquire more deeply into the action of light upon growth, and the first problem which presents itself is to find what rays of the solar spectrum are particularly concerned in it. We might obtain the solution of this problem by exposing the growing parts of a plant successively to the action of the rays of different refrangibility which make up the solar spectrum, and by comparing the rates of growth observed. We should find that the rapidity of growth would gradually diminish as the plant was removed from the red towards the blue end of the spectrum, until it entirely ceased. This mode of experiment is, however, scarcely suitable for the general purposes of demonstration, as it must be carried out with

great care, and involves the use of a good deal of apparatus. The same results may be more easily arrived at by means of these two flasks which I have on the table, and which were devised by Prof. Sachs, of the University of Wurzburg (No. 3944). They are simply large glass bottles with double walls, the space between the walls of the one being filled with an ammoniacal solution of copper oxide, and of the other, with a solution of potassium bichromate—the former being of a blue, the latter of an orange colour. In this way we are enabled to split up the solar spectrum into two halves,—the one half of its rays (including part of the green, the blue, indigo, violet, and the actinic rays) penetrates into the cavity of the blue flask—the other half (including part of the green, the yellow, orange, red, and dark rays) reaches the interior of the yellow flask. That the properties of these two sets of rays are very different is indicated by the fact that a piece of sensitive paper, such as is used in photography, rapidly becomes darkened when exposed in the blue flask, whereas it is scarcely affected by exposure in the yellow one; and we may therefore conclude that, in the former case, we have to deal with rays of considerable actinic power, whereas, in the latter, the rays are comparatively inert. Let us see how these two sets of rays affect the process of growth. Here are some young pea-plants which have been grown for some days inside these flasks, and have been supplied equally with air and moisture. It will strike you at once the growth has taken place much more rapidly under the influence of yellow than under that of blue light, for there is a difference of several inches between the heights of the plants. From this we may conclude that the rays of high refrangibility are more active in arresting growth than those of lower refrangibility.

A careful examination of these two plants will show that they differ not only in the amount, but also in the direction of their growth. You see that this plant grown in the blue flask has become considerably curved in a definite direction, and this was towards the source of its illumination, whereas the plant grown in the yellow flask is nearly straight. This tendency to bend towards the source of light is due, then, to the action of the highly refrangible rays upon the growing cells. These rays have

a more powerful action in retarding the growth of those cells of the stem which are more immediately exposed to them than they have in retarding that of the cells more removed from their influence, and consequently the stem becomes bent towards the source of light.

To the phenomena resulting from the action of light upon the process of growth the general term *heliotropism* is applied. This young pea-plant, then, affords an example of heliotropism, and since the direction of its curvature is towards the source of light, the heliotropism is said to be positive. It does happen, however, that in some plants and parts of plants the direction of curvature is away from the source of light (for instance, in the tendrils of Vitis and Ampelopsis and many aërial roots), and under these circumstances the heliotropism is said to be negative. The explanation of this negative heliotropism is by no means satisfactory as yet. Positive heliotropism we said, depends upon a retardation of the growth of the cells of the more brightly illuminated side of the stem, and we might be inclined to go on to say that negative heliotropism depends upon an increased rapidity of the growth of the cells upon the more brightly illuminated side—to admit, in fact, that the cells of some plants have their growth diminished by the action of light, whereas the cells of others have their growth increased. An attempt has been made to explain the negative heliotropism of tolerable transparent structures (*e.g.* the aërial roots of Aroids) by showing that in such cases the side most removed from the source of light receives a more intense illumination than the side nearest to it in consequence of the refractions which the rays of light undergo after their penetration into the tissues of the organ (Wolkoff), and under these circumstances, the apparent negative heliotropism of these organs would be merely a special case of positive heliotropism. It is evident, however, that this theory is quite inapplicable to the cases of negative heliotropism occurring in organs which are not transparent. The true explanation of negative heliotropism has yet, I believe, to be discovered. For the present we may perhaps be contented to assume provisionally the hypothesis that growing cells are of two kinds, the cells of the one kind having the rapidity of their growth increased, those of the

other having the rapidity of their growth diminished by the action of light,—that there are, in fact, cells which are positively or negatively heliotropic, just as there are cells which are positively or negatively geotropic.

Growth is, however, only one of the processes of plant life which are influenced by the mechanical action of the more refrangible rays of the spectrum. For instance, the movements of zoospores,[1] as well as those of the motile parts of the higher plants, are all influenced by these rays; and we may say generally that the more purely physical phenomena of plant life are especially affected by them.

The less refrangible rays—such as would penetrate into the interior of this orange-coloured flask—are concerned with the chemical processes of plants. Of the truth of this statement I am unable on this occasion to give you any demonstration. I can, however, refer you to experiments which show conclusively that some of the most important chemical processes can only take place in the presence of such rays. Assimilation, for instance—that process in which the carbonic acid gas present in the air is decomposed by the green colouring matter of the leaves, and the carbon thus obtained is combined with oxygen and hydrogen derived from water to form starch, whilst at the same time oxygen is liberated—can only take place under the influence of these rays of low refrangibility, the yellow rays being particularly active in promoting this process (Draper; Pfeffer).

With the apparatus of Prof. Sachs our list closes. The instruments exhibited are few in number and are comparatively simple in construction, but they are of considerable interest, for the application of them has materially contributed to the solution of some of the more important and difficult problems of Vegetable Physiology.

[1] Since the above was written Prof. Sachs has published a paper, in which he shows that the apparent heliotropism of zoospores is produced by currents in the water, in consequence of slight differences of temperature in different parts.

ELECTRICAL MEASUREMENTS.

TWO LECTURES.

BY PROF. CAREY FOSTER.

LECTURE I.

UNDER the circumstances of these Lectures I think it will probably be most useful, if, instead of attempting to describe the details of the construction or use of special instruments, I try to explain as well as I can in the time available, the principles upon which the use of instruments of any general class depends. You are aware that the subject of the present lecture, and the one to follow next week is Electrical Measurement, and I shall to-day speak of measurements which have relation to Statical Electricity or electrostatic measurements. Here the magnitudes which have to be measured are—the *Quantity* of electricity, the *Potential* of the electricity, and the electrical *Capacity* of the bodies which may be charged. Then we have also the Energy represented by a quantity of accumulated electricity or a given electric charge. These are the fundamental magnitudes with which we have to deal in electrostatics; but there are various secondary matters which may be made a subject of measurement, for instance electrical Density, which is the quantity of electricity per unit of volume. If we have any electrified substance the quantity of electricity in unit of volume of that body is the density of the charge; if we have a charged conductor, then, as is well known, the electric charge is confined

to the surface so that we have not to deal with volume in the case of conducting bodies but with surface only. The superficial density is the quantity of electricity per unit of surface. Thus electrical Density is a subsidiary magnitude connected with Quantity. Then in connection with Capacity there is a magnitude which may be regarded as a subsidiary one. You are well aware that the capacity of a given conductor, of this cylinder for instance, that is the quantity of electricity which it must take up in order to have a charge of given potential, depends partly on the form and partly on the dimensions, but also on the distance of surrounding bodies and on the nature of the substance which separates the conductor we are discussing from the surrounding bodies. There is then a property of the surrounding medium which has something to do with determining the capacity of a given conductor. This cylinder for instance is surrounded by conductors such as the walls of the room, the ceiling, the table, and so on, as well as by other nearer bodies, and it is separated from these conductors by air. If we were to replace the air by another insulating medium, the electrical capacity of the cylinder would be changed; and that property of the surrounding medium which enables it to produce an effect on the capacity of a given conductor is called the *specific inductive capacity* of the medium or the *dielectric co-efficient*, these being synonymous names for the same property.

The measurement of these different magnitudes constitutes the problem that we have to discuss in talking of electrostatic measurements. In the case of any measurement we may adopt what we may call a direct process, or an indirect one, and to make more clear what I mean by this distinction I may remind you of familiar processes in other cases. I call a direct measurement one such as an ordinary measurement of length. If we want to measure the length of this table we take a foot-rule or any other standard we may agree upon, and by measuring off this standard length over and over again we estimate the length of the table in terms of the adopted unit: but in the case of electricity such a process as this is not available. One cannot take a standard quantity of electricity and compare this by a direct process with any other quantity that we want to measure. We have to adopt a process less direct, in which we observe the effect which the

quantity of electricity which we have to measure can produce, and compare this with the magnitude of the similar effect produced by the standard quantity. This rather abstract way of stating the case will be more intelligible if I give you an example. In the case, for instance, of heat we are pretty much in the same circumstances as in relation to the measurement of electricity. We cannot take a unit of heat like a unit of length and compare it with other quantities. We have to produce some effect by the quantity of heat we want to measure. We may for instance employ it to raise the temperature of water from the melting-point of ice to one degree and estimate the quantity of heat by the amount of water whose temperature can so be raised from zero to 1° C, or we may use the heat in melting ice and estimate the quantity of heat by the quantity of ice that it can melt, the unit of heat being of course either that quantity which can raise a unit mass of water one degree, or that which would melt a unit mass of ice.

So in the case of electricity, we may adopt as a standard quantity, as the unit of electricity, the quantity which can produce some definite effect, and then to measure other quantities we find out how much of the same effect they can produce. If we want to get what we may term an absolute measure some such process as this is always necessary, but where what we require is simply to compare one quantity with another without requiring to know absolutely what either of them is—if we want to know for instance that one quantity is 100 times another without knowing how much this one is—for mere comparisons of that sort, many other processes are available: for instance, we may adopt, as giving a comparative measure of two quantities of electricity, the number of times which some definite operation has to be repeated in order to produce the one quantity or the other. The number of turns of the handle of a given machine for instance, might be taken as a measure of the quantity of electricity which is employed in a particular experiment. If we turn the handle 10 times we produce a definite quantity: if we turn it 20 times we produce twice as much, when the machine is in the same condition; but as all of you know who have worked with electrical apparatus, one day a machine will give us much larger quantities than it will another day, when employed in the same manner, so that such a method

would not be available for comparisons except in experiments made very quickly one after the other. Again, the Electrophorus, every time we lift the cover and take a spark from it, gives us the same quantity of electricity; the cover always comes up with the same quantity, so that we might charge a conductor from the cover of the electrophorus by giving it several sparks one after the other, and we might estimate the charge by counting the number of sparks which were given. That at first sight seems, as far as it goes, a perfectly satisfactory method of measurement; but, on consideration, you will see it is not quite so good as it appears, because although the electrophorus possesses each time the same quantity of electricity, it does not give the same quantity each time to the conductor with which we put it into contact. To take a definite example, suppose that when I let the electrophorus touch this sphere it gives up half its electricity to the sphere: then I charge it again and I put it in contact again, it does not give up so much as before. To begin with, it came in contact with an uncharged sphere, but now it comes in contact with a sphere which has already half as much electricity as the electrophorus itself, so that at each repetition of the process we give a smaller and smaller quantity to the sphere, and you will very easily see that on repeated contacts the quantities of electricity imparted to a good conductor diminish according to the terms of a decreasing geometrical series. It is clear that the more nearly the potential of the sphere agrees with the potential of the electrophorus, the less is the quantity of electricity which passes from one to the other; and the same thing applies in the case of an electrical machine. If we connect a machine with a Leyden jar or with a number of Leyden jars, one turn of the handle will cause a certain quantity of electricity to pass into the jar; another turn will cause almost the same quantity to pass in, but you know that after a time the jar gets as strong a charge as the machine we employ can give to it, and then we may go on turning the machine as much as we like, and the charge in the jar does not increase. Hence the proportionality between the charge in the Leyden battery and the number of turns of the machine only holds good at first. If we go on turning we come to a time when we do not increase the charge at all. The quantity produced is however exactly proportional to the number of turns, if the machine remains

in the same state. If instead of charging a battery or other insulated conductor we allow the charge to pass off to the earth, we get for each turn of the handle a definite number of sparks, each spark corresponding to the passage of a definite quantity of electricity. The quantity produced by each turn of the machine is definite, but the quantity which will go into the battery becomes smaller and smaller as the quantity already in the battery increases. Another way of measuring out the charge for a Leyden battery or any such apparatus is by the employment of a unit jar, one familiar form of which I have here upon the table. The quantity which the second jar receives is measured by the number of sparks which pass into the knob connected with the first, but the point I want to draw attention to is, that the sparks indicate that a definite quantity has passed into the second jar. By the time the first spark comes, a certain quantity has passed into this jar and by the time the second spark comes the same quantity has passed in again; so that the action of the unit jar is comparable to the case of lading water into a cask out of a measure of definite capacity. The spark is the signal indicating that a certain definite quantity has gone in. It is not that the electricity goes in at the moment of a spark, but the number of sparks counts *the number of times which the first jar is emptied into the* second, so that when we want to give three units to the second jar, we ought to break the connection as soon as the third spark has passed, not to go on until the fourth spark is nearly occurring.

All such measurements as these I have referred to are merely comparative; if we want to know, not merely what is the proportion between one quantity of electricity and another, but what the actual quantity is in any particular case, then we must employ a method founded upon the action which the given quantity can produce, and compare this with the amount of the same kind of action which the adopted unit of electricity could produce. The most obvious way of getting an absolute measure of quantities of electricity is founded upon the law established by Coulomb, which applies to the force exerted between two quantities of electricity. If we have two equal quantities of electricity, represented each by q then they attract each other if they are of an opposite kind or repel each other if they are

similar, with a force represented by this formula $f = \frac{q^2}{r^2}$. The force is numerically equal to the product of the two charges, or to the square of either of them if they are equal, divided by the square of the distance between the points at which they are collected. q stands for the amount of the charge or quantity of electricity, r for the distance between the charges and f for the force. Reading the relation differently we have $q = r\sqrt{f}$; or the measure of the charge is the product of the distance into the square root of the force exerted between the two charges. This formula implies that the quantity of electricity which, placed at unit distance from another equal quantity, repels it with unit force, is taken as the standard quantity of electricity; so that the unit charge or absolute unit of electricity is the charge which repels an equal charge at unit distance (say one centimetre) with unit force (one dyne). The dyne is the most convenient unit of force; it is that force which can in one second give to a mass of one gramme, a velocity of one centimetre per second. But that is not a matter which is essential to our present discussion. You may take what unit you please as the unit of force, but the unit of electricity will depend on that.

To return to what I was saying: in order to apply practically a system of measurement founded upon this principle, it is most convenient, although not absolutely essential, that the charges that we want to measure should be situated upon spherical conductors, little spheres of metal, or spheres with metal surfaces. The advantage of this form of a charged body is this, that the electricity distributes itself uniformly on the surface of a sphere if that is at a sufficient distance from other bodies, and this uniformly spherical layer of electricity acts upon other electricity as though it were concentrated at the centre of the sphere; so that we may in this case regard the quantities which exist upon the spheres as concentrated at their centres. If they are at a very great distance apart that is strictly true; if at a moderate distance apart it is nearly true. The apparatus in most general use for this purpose is the Torsion Balance which is fully described in the ordinary Text-books on Electricity, and of which I have an example here. In using it, a charge of

electricity is given to the fixed ball; then if the movable ball is of the same size as the fixed one, the charge of the fixed ball is divided between the two in equal proportions. The balls then repel each other with a force proportional to the square of the charge upon either of them, that is, proportional to one fourth of the square of the original charge. If we measure the force exerted between the two balls each of them charged with half the original quantity put into the apparatus, which we can do by observing the extent to which the suspending fibre is twisted and determining its coefficient of torsion, and measure the distance between the balls, which we can do by observing the angle between them and the length of the movable arm, we get the two factors which determine the quantity upon each ball. Then the square of that is equal to one-fourth the square of the original charge. The details of the use of the apparatus would call for a good many remarks if we had time to enter upon it, but that I cannot do now.

So far as I have spoken of this apparatus it enables us to measure the quantity of electricity which we put into it upon the fixed ball, but that does not yet give us a method of measuring quantities of electricity in general. If I wanted to measure, for instance, the charge of this sphere, I still might do it by means of this apparatus. I should take the fixed ball out of the balance, let it touch the sphere, and then put it in and measure the charge which it has got. The charge taken from the sphere would bear to the charge that the sphere had to begin with, a definite relation which is not, as one might suppose at first sight, the ratio of the surfaces of the spheres. That you will easily see in this way: If we touch this large sphere with a small one, you might suppose that the charge of the large sphere would be divided between the two in the ratio of the surfaces of the large sphere and of the small one; but a little consideration shows that that is not the case, for the density of the charge upon the small sphere is greater than the density upon the large sphere. It varies from one part to another, but the average density on the small one is considerably greater than the average density upon the large one, so that the small sphere takes under these circumstances a larger proportion than corresponds merely to its surface, and the ratio in which the electricity divides between the two spheres bears a complicated relation to their sizes. But tables

have been calculated by reference to which the proportion in which the electricity divides can generally be easily ascertained. Such facts, however, are applicable only to the case of the partition of electricity between spherical conductors; but we may make any conductor virtually spherical by putting it inside a hollow sphere. If I charge this ball and then hold it inside this jar by an insulating fibre, the outside of the jar, although the ball does not touch it at all, and there is perfect insulation between them, assumes a charge which is just equal to the charge of the ball; the inner surface of the jar assumes a charge which is equal but opposite to the charge of the ball, and the outer surface gets a charge which is equal and similar to that of the ball; so that even without any actual contact we get a charge outside the jar equal to the charge of any body which is inside. It does not matter whether contact takes place or not. A charged body inside a perfectly closed *conductor causes on the outside of that conductor a charge* exactly equal to its own. If we had better insulation it would be easy by a simple experiment to prove this, but I am afraid I could not in the present state of the atmosphere, and with the probable dampness of the supports, make the experiment in a satisfactory way, and therefore I will not attempt it. The experiment is this: You charge the ball *and put it inside the jar without letting it come in contact.* Then you touch the outside of the jar, and so render it neutral. If you now take out the ball without allowing contact to occur, although the whole apparatus is neutral as long as the ball is inside, it is not neutral when the ball is taken out; but if before removing the ball you let it touch the jar—which should be done without interfering with the insulation of the jar —and then take it out, you will find the apparatus is still neutral. This shews that the electricity which escapes to the ground, when the jar is touched while the ball hangs inside, is equal in quantity to, and of the same kind as, the charge of the ball; for, after its escape, the jar is rendered permanently neutral by receiving the electricity of the ball. On the other hand, if the jar remains insulated while the ball is inside it, its outer surface retains the electricity which, when it is uninsulated, passes away to the earth,—that is, it retains a charge equal and similar to that of the ball. However, independently of any particular experiment to prove the point, this is a prin-

ciple of perfectly general application, that if we have one or any number of electrified bodies completely inclosed in a hollow conductor which is insulated and possesses no electrification independent of the bodies inclosed, the outside of the conductor possesses a charge equal to the sum of all the charges which are inside. Therefore if this conductor is a sphere we have on the outside of that sphere a quantity of electricity exactly equal to all that is inside, and we may measure that quantity by allowing it to be divided between the hollow sphere and a small external one which can be put into the balance. Thus in a very great number of cases the measurement of the quantity of electricity in any electrified body can be reduced to the measurement of the electricity of a charged sphere.

We have next to speak of Density, or the quantity of electricity on unit of surface. As you are aware, the general method of measuring this is to touch the conductor, on which we want to know the density by a small plane conductor. If the conductor with which we touch the body to be tested is small enough, the outer surface of the plane conductor—which may conveniently be a small disc of gilt cardboard—becomes, for the time being, practically part of the surface of the body to be examined, so that the quantity of electricity which was previously upon the part of the surface covered by the gilt cardboard is transferred to the cardboard. When we take this away we carry off the quantity of electricity previously on that part of the surface, and can measure it in a torsion-balance; we can also ascertain the amount of surface upon which it was; then, having a measured quantity and measured surface, the ratio of quantity to surface gives the average density. There is another method which may be employed in special cases for determining the density of a charge. It is founded on this principle, which I have not time to prove, but which is probably known to many of you, that the electrical force just outside a charged conductor is equal to four times π (in its ordinary meaning as representing the ratio of the circumference of a circle to the diameter) multiplied by the electrical density.

In speaking of the electric force just outside a conductor I mean this: Imagine a unit of electricity close to the surface of the conductor, and suppose it can be put there without disturbing the actual distribution of electricity previously

existing, then the force exerted on that unit of electricity is the force which we have to deal with in this case. If we can-measure that force we can measure the density. This method, however, is not generally applicable, because we cannot usually place a charge outside a conductor without disturbing the condition of the conductor itself. The generally applicable method is that by means of the carrier-plane, which I spoke of before.

The next fundamental magnitude is electrical Potential. This, in the case of electricity, corresponds to temperature in the case of heat, or to the surface level in the case of a liquid. The properties of any charged conductor depend not only on the quantity of electricity it contains, but also upon what we may call in general the quality of the electricity, or the potential. Just so the properties of a body containing heat depend not only on the quantity of heat in the body, but upon the temperature of that heat, or the temperature of the body as we call it. Potential is a magnitude of which we cannot avoid the discussion in speaking of electrical phenomena, for it corresponds to properties which are not expressed by any other term. The potential of a conductor, or rather the potentials of two conductors, determine whether, when they are connected together, there is any passage of electricity from one to the other, and there is no other property which in general determines this. It is easy to convince ourselves that whether electricity passes from one conductor to another or not, does not depend simply on the quantity which either of them contains. Electricity does not always go from a conductor which contains a large quantity to one which contains a small quantity. Suppose this small ball and a larger sphere brought together, the sphere being charged to begin with; put the ball in contact with the sphere, and then separate them. They are now both charged, and the sphere has got more electricity than the ball, but if you put them in contact again there is no further discharge of electricity from one to the other. We have unequal quantities, but we have not, therefore, a passage of electricity from one to the other. Again, it is not a question of the density of the charge, for we have a greater density on this small ball than on the sphere; still when we make the contact no electricity passes. A still more conclusive case of the same kind is this. Take an

elongated conductor such as a cylinder, or still better one which is rounded at one end and pointed at the other. You know that the electrical density at the extremities is greater than at the intermediate parts, and if one end is rounded and the other has a sharp point, the density at the point is very much greater than the density at the round end; *still* the electricity does not flow from the point to the round end, or from the ends to the middle. Take again such a case as this. If we charge this jar inside and touch the inside with a carrier-plane, we cannot detect any electrification; if we touch the outside we find it electrified. If you charge the inside or outside of the jar, and then examine the inside, you get no charge from that, but you do get a charge from the outside, so that examining it in this way the inside of the jar appears to be neutral. We connect the jar with the electroscope in a different way by taking an insulated wire, and bringing one end in contact with the outside of the jar; the leaves of the electroscope diverge, showing that the jar is charged. Bring the end of the wire inside the jar, and the leaves will likewise diverge. Thus, the inside of the jar examined by means of a carrier-plane appears to be neutral, but examined by means of a wire connected with an electroscope, it appears to be charged just as much as the outside. Again, although you cannot discharge the jar by lading out electricity from the inside by a carrier, you can discharge it by touching the outside with a carrier, then uninsulating the carrier, touching the jar again, and repeating the process over and over again. You can discharge the jar in this way from the outside but not from the inside; but if you use a wire connected with the earth, whether you touch the inside or touch the outside of the jar, you equally discharge it.

There is another way in which we can show that the inside of the jar although there is no electric density upon it, and it appears neutral, when we examine it by means of a carrier it is not in the same condition as a space not surrounded by electricity. If we put in an insulated ball which is neutral to begin with, the jar being charged on the outside, and, when the ball is inside, touch the ball with a wire in connection with the earth and then take it out, we find the ball charged, although there is no charge on the inside of the jar. Thus, although the space within the jar does not

contain any electricity, it is not similar to a perfectly neutral space. In fact the condition of the jar can only be expressed by means of the term potential. If the jar is charged with positive electricity the potential of the inside is higher than the potential of the space outside. One result of this is that positive electricity tends to pass away from any conductor put inside the jar, and will pass away if there is a communication to allow it to do so. Electricity always tends to pass from points of high potential, to points of lower potential just as heat tends to pass from points of high temperature to points of lower temperature.

The precise definition of potential at any point may be stated in this way: Suppose we have a disc charged with a unit of positive electricity, and we bring it to the point in question from a place where no electric force acts, that is from a great distance from any charged conductor. To make sure that we begin far enough away, we start at an infinite distance, and carry the unit of electricity up to the given position. The total amount of work that we have done in bringing the electricity to that position is the potential of the place that we bring it to. If we bring it into a conductor it is the potential of the conductor. The electrical potential of any point is the total amount of work we must do to bring to that point a unit of electricity from a position so far off that no electric force acts at the point we bring it from. Instruments for measuring potential and differences of potential are called Electrometers, to which class of instruments two subsequent lectures are to be devoted, so that I will not enter further upon this question.

The next magnitude is the Capacity of conductors. Electrical capacity is exactly comparable to the capacity for heat of any body, or to capacity in its ordinary sense. The capacity of a vessel for liquid is the quantity of liquid which it can contain, only in the case of electricity the quantity which a conductor can hold depends upon the means that we employ for putting electricity into it. The capacity of a conductor is perhaps better compared, not to the capacity of a vessel for liquid, but to its capacity for a gas. The quantity of air we can put into any space depends upon the pressure of the air upon it. Without increasing the space we can put twice as much air in if we double the pressure. So in the case of a particular conductor, we can put twice as much

electricity into it if we double its potential. The electrical capacity of the conductor is the quantity of electricity required to produce unit change of potential. If the potential of the conductor be zero before we give to it the charge, then the quantity required to charge it to unit potential measures its capacity. In general terms, if C stands for the capacity of a conductor, and if the quantity Q of electricity given to that conductor increases its potential by the amount V, then $C = \frac{Q}{V}$, and $\frac{Q}{V}$ would be the quantity required to increase its potential by unity. Or, to state the relation in another way, the total quantity of electricity a conductor contains equals the product of its capacity into the potential of the charge, or $Q = CV$, just as the quantity of gas in a vessel might be measured by the product of the volume into the pressure.

From this relation you see that if we know the quantity and potential or their ratio, we know the capacity; if we know the capacity and potential, we know the quantity; or again if we know the capacity and the quantity we can determine the potential, so that the measurement of these three things cannot be separated from each other. The quantity of electricity in a conductor can be measured by one or other of the processes I have indicated, and its potential can be measured by electrometers, and the ratio of these measures the capacity. That is the general method of absolute measurement. When we have merely to compare different quantities, we may employ methods analogous to those employed for measuring capacities for heat. For instance, to determine the specific heat, or capacity for heat, of a substance we put this substance at a known temperature into a known mass of water at another known temperature, and observe the changes of temperature of the two. The temperature of the immersed body falls and that of the water rises, and they come to equilibrium at some intermediate point. The same quantity of heat is lost by one that is received by the other, and the capacities for heat of the immersed body and of the water are inversely proportional to the changes of temperature which they respectively undergo. So in comparing the capacities of two electrical conductors: suppose we have one of them charged to the potential V, and the other at zero or uncharged, then, when we allow them to come into contact, they both assume a common potential,

say v, intermediate between these two. One body has lost the same quantity of electricity that the other has received. The potential in one case has fallen through $V - v$, and in the other it has risen through $0 + v$ and the capacities are in the inverse ratio of the changes of potential caused by the loss or gain of the same quantity of electricity; so that if the first body has the capacity C, and the second body the capacity c, the capacities are to each other in the inverse ratio of the changes of potential, or $\frac{c}{C} = \frac{V - v}{v}$. That is the principle of a great number of methods of comparing the capacities of different conductors. There is not enough time to enter on the experiment in detail, or even on the principle of other methods which might be given. If we have the capacity of any one conductor measured in absolute measure, the capacity of others can also be determined in absolute measure by some process of comparison such as I have indicated; if we know the absolute value of one thing, and can compare others with it, we know their absolute value also. Upon the table are standard condensers or accumulators, as they are better called. They are conductors of carefully ascertained capacity. They are of different forms and different values. One is a very large one formed practically by a combination of small ones. Here again is an instrument devised by Sir William Thomson which is a conductor of variable capacity; its capacity can be diminished by drawing out the cylinder, and increased by pushing it in, so that it can be adjusted so as to have the same capacity as any particular conductor we have to measure and then we can read off on the scale the capacity of the other one.

The Energy of charged conductors is proportional both to the quantity and to the potential; the absolute energy of a charge of electricity is equal to half the product of the charge into the potential, or $\frac{1}{2}$ QV = E. That can be proved by an apparatus like this. You discharge the electricity through a fine wire stretched across the bulb of an air thermometer and observe the amount of heat. This gives us a quantity which is proportional to the energy of the charge.

I must be content with merely indicating these points; and in the lecture to follow on Wednesday next I shall have to speak of some of the chief measurements connected with electrical currents or dynamical electricity.

ELECTRICAL MEASUREMENTS.

LECTURE II.

THE measurements connected with electrical currents—dynamical electricity—are chiefly three. We have—1. The measurement of the Strength of Currents, or, to express it more shortly, the measurement of electric currents; 2. The measurement of Electromotive Force; and 3. The measurement of the Resistance of Conductors. The measurement of resistance or of conducting power comes to the same thing, for the relation between these two properties is a reciprocal one. If we know the conducting power of any piece of wire or other conductor, we know that the resistance is equal to 1 ÷ the conducting power; or the conducting power is equal to 1 ÷ the resistance; so that if the conducting power of a wire is one-half, its resistance is two; and if the resistance is one-half, the conducting power is two, and so on. One is the reciprocal of the other, so that whether we speak of the measurement of resistance or of conducting power it is essentially the same thing. A statement of the one is a statement in another form of the other.

What I shall try to explain to-day, as far as time and circumstances permit, is the methods by which absolute measures of these three quantities can be obtained. When I speak of the absolute measure of a current, I mean a measurement which tells us, not that one current, for instance, is twice or three times as strong as some other current, but which tells us the actual quantity of electricity conveyed by the current in unit of time. The idea attached to the term strength of current in the case of electricity is exactly comparable to the idea attached to

the corresponding term in the case of a current of anything else. If we are speaking of a current of water, we mean by the strength of a current the quantity of water delivered in a given time, say a minute. Similarly the quantity of electricity conveyed along a conductor by a given current in a second or minute is the strength of the current. Very frequently, however, processes are spoken of as measurements of the strength of currents which really enable us merely to compare one current with another. They are comparative measures, but not absolute measures. We might have several water pipes delivering water each at its own rate, and we might possibly ascertain that one delivers three times as much per minute as another, and another one sixteen times as much as the first, without knowing what is the absolute quantity delivered by either of them, and we have in electrical, as in all other measurements, to *distinguish between such comparative* measurements and absolute measurements which tell us, not only whether a thing is greater or less than some other thing of the same kind, but actually how great it is. And it is the general principles of the methods by which absolute measurements of the strength of currents can be obtained that I shall have to speak of.

There are three chief properties or three effects produced by electric currents, any one of which might be made the basis of a system of measurement. You all know that when an electric current passes near a magnetic needle it exerts force upon the needle, and usually displaces the needle from its ordinary position; a current passing near a magnet deflects it, so that we may say that an electric current produces electro magnetic effects; and we may use the term electro magnetic action of a current to include all the mutual effects which take place between currents and magnets. Upon this action of electric currents then we might found a system of absolute measurement. Then, again, one electric current passing near another electric current exerts a force upon it, or rather when two currents are near each other there is in general a mutual force tending to displace them relatively to each other. The forces exerted by electric currents upon each other, usually spoken of as electro-dynamic forces, might serve as the basis of a second system of

measurement. Again, an electric current passed through a compound liquid almost always causes chemical change—a decomposition of the substance. The general name given to processes of this kind is electrolysis—the electrical breaking up of the substance. And the electrolytic action of currents might be employed to furnish a third method of measuring their strength.

In order to employ any one of these principles for the measurement of currents, the conditions under which these actions take place must be rendered as simple and definite as possible. Without going into minute particulars I may remind you—to take the electro magnetic action first—that the force exerted by an electric current upon a magnet depends partly on the strength of the current, partly upon the relative positions of the magnet and of the conductor in which the current passes. This I may almost say is self-evident, that a change of relative position would change the effect. One kind of change of relative position would be a change of distance: the farther a current is from a magnet, obviously the less it will act upon it; but independently of changes of distance, a change of direction will in general cause a change of action; so that, to express the matter in general terms, we may say that the effect of a current upon a magnet depends on the relative positions of the two. It also depends upon the length of the conductor which is in the neighbourhood of the magnet. That, in a certain sense, is merely a repetition of what I said as to position, and will be evident at once. If you take, for instance, what I have here—a magnet suspended on a pivot at the centre of a graduated circle, with a circular conductor formed by a coil of wire which would enable us to pass a current many times round the magnet, and terminated by wires by which it can be connected with external conductors—if those wires are carried far away they may go to such a distance that the distant portion of them does not exert any sensible influence on the magnet. So that we have to consider the length of the conductor near the magnet, not the total length of the conducting wire. We might have a current sent through this coil from a mile or 100 miles away, but the portion of the conductor at that distance would obviously exert no sensible effect. So then, the force exerted upon the magnet depends

upon the strength of the current, the relative positions of the current and magnet, and the length of the conductor which is within acting range of the magnet. The effect also depends upon the strength of the magnet itself; but we may consider that we always deal with a magnet of unit strength, and may therefore leave it out of consideration now. The simplest rule to adopt to obtain a standard of measurement founded upon this action, is then to say that we will call that current a current of unit strength, of which a unit length placed at *unit distance from any given point causes unit magnetic* force at that point. If you consider that statement you will see at once some of the conditions that are necessary in order to apply a system of measurement founded upon the principle we are dealing with. If we are to have a given length of a conductor, all of it at some definite distance from a particular point, it follows at once that the conductor must be bent into a circular arc. If we have a line and every point of this line is to be at the same distance from some particular point, the line must not be straight, but it must be curved into the arc of a circle of which this point is the centre. In order not to complicate the matter by using unusual units, I will suppose we take a foot as the unit of length. Take a wire a foot long and bend it into the arc of a circle of one foot radius, so that each point of it is at one foot distance from the centre. Then suppose that we have an electric current flowing along this wire; this current will cause any magnet near it to be acted upon by a force in one direction or the other, and suppose that we have a magnet at the centre point. If the magnetic force exerted at this particular point by the current is the force one, say a force equal to the weight of one grain, the strength of the current which flows along the conductor is the standard of measurement—it is represented by unity. And if the force exerted here is 2, the strength of the current also is represented by 2; or *if the force at the centre is 100, the strength of the current* is 100. So that we take as the standard of measurement of the strength of the current, the magnetic force exerted at the centre of a circle of unit radius by unit length of the current bent round the circumference of the circle. Practically, this precise arrangement would not be con-

venient, and, therefore, instead of taking a wire, say one foot long, and putting that partly round the circumference of a circle of a foot radius we take a complete circumference ; that is, if the radius of the circle is 1, the circumference would be about 6·28 ; but the magnetic force at the centre of the circle will be just proportional to the length of the conductor. If we take one foot round a circle then unit current would exert unit force; but if we take six feet round the circle then unit current in the circumference would exert six times unit force; so we consider that unit current is the current which, going once round a circle of unit radius, exerts at the centre, not unit force, but 6·28 units of force.

This principle is adopted in all such instruments as these, where we have a current carried once round or several times round a circle. If I send a current round this circle I shall be able to cause a deflection of the magnet at the centre. You will see from what I have said that the problem we have to deal with in measuring an electric current by the method I have been speaking of, is to take a measured length of the current at a measured distance from a definite point, and to measure the magnetic force exerted at that point. The easiest way of measuring the magnetic force exerted at the centre of the circle is to observe the deflection from the natural position which a small magnet placed at the centre undergoes. If we know the magnetic force exerted at any given point due to the magnetic force of the earth, then it is easy to ascertain what is the force exerted on the magnet when it is deflected through a measured angle from the position it would take if acted upon by the force of the earth only. We have a little magnet at the centre of the circle, acted upon by the earth, and it takes a definite position in consequence. If we act upon it also by an electric current we in general displace it from the position it would take if acted on by the earth only, and if we know the force exerted upon the magnet by the earth, and if we know also the extent to which it is deflected by an electric current, then we can measure the force exerted by the electric current. In that way we get to know what the force is which the current exerts ; by measuring the radius of the circle, and the number of times which the wire goes round, we get the

length of the current which acts upon the magnet as well as its distance from it; and in this way we have the measurement complete.

The apparatus which may be used for applying a measurement upon these principles may receive very various forms. This is one of an essentially very simple kind. It has the advantage that it can be employed to measure currents of very different degrees of strength. If we have a very strong current it may deflect a magnet so much that we could not readily deduce the amount of force exerted. If there is too great a force applied to any particular instrument its indications are not so certain. We have a familiar example of this in the case of a spring balance. It will indicate, perhaps, very accurately, weights up to a certain amount, but if you put on too great a weight then the indication becomes uncertain. So any particular measuring instrument acts best for quantities which lie within a certain range; and it generally requires a different instrument or different adjustment of the same instrument to measure quantities differing greatly from one another. I may just point out the way in which this particular instrument may be modified to suit stronger currents than those which act upon it as now arranged; that is by displacing the circular conductor from the magnet. The magnet is not at the centre of the circle, but each portion of the wire, instead of surrounding the magnet, is on the surface of a cone, of which the magnet is at the apex, and the further we move it in this direction the smaller becomes the effect of each portion of the wire, and therefore the stronger the current required to give a definite deflection.

Here is another form of apparatus for the same purpose. It is essentially the same instrument although smaller in size and in various ways differently arranged. There is a point at the centre for carrying a small magnet, a graduated circle to show how much the magnet is deflected, and a vertical circle in which the current is to pass. Most of you are aware that in using an instrument such as either of these, it must be placed in a certain position; that the plane containing the circular conductor must coincide with what is called the plane of the magnetic meridian. The circle must in the first place be vertical, and it must also coincide with the plane of the earth's magnetic force,

That in this room coincides pretty nearly with the length of the room. The larger instrument is, therefore, improperly placed, being across the room, so that it would have to be turned round. The circle being properly set, you know also that the strength of the current is in proportion, not to the deflection of the magnet, but to the tangent of that deflection; so that if the magnet is deflected through any angle, the strength of the current is proportional to the tangent of this angle. Instruments such as these are therefore usually termed Tangent Galvanometers; galvanometer being the name given to any instrument which measures the strength of a current by the electro-magnetic action of that current.

Here is an instrument, one of the first, I believe, of the kind which was constructed; it would not however, easily give us an absolute measurement. There is a magnet and a coil of wire surrounding it, so that when a current passes through the coil the magnet is deflected; but the section of the coil which is wound on this elongated reel is not circular, so that it would involve a very complicated calculation in order to ascertain the mean distance of the various parts of the current from the magnet. Such an instrument could hardly be made to serve for absolute measurement, but it will serve very well for comparative measurements, and for that purpose it is so arranged that the coil can turn independently of the magnet, and it is used in this way:—If we send a current through the coil and that deflects the magnet, then the coil and the plate which carries it are turned round until a fixed mark is brought to coincide with an index attached to the magnet, and the measurement depends on the extent to which this circle is turned. It is an instrument which will give very accurate comparative measurements, but not absolute measurements. On the table there are various forms of instruments for giving comparative measurements, and one also for giving absolute measurements. There is a magnet suspended inside this copper-box, and it is in the form of a magnetized steel ring. The thick mass of copper which surrounds it is for the purpose of making it come rapidly to rest when displaced. Probably you know that when a magnet swings in the neighbourhood of a large mass of copper or other conducting material, its

movement is damped, just as if it were swinging in oil or some resisting fluid. Here is a circular plate with a groove cut in it, in which a conducting wire can be placed, and the diameter of the circle can be accurately measured. The distance of the circle from the magnet is also accurately measured by means of a divided scale. Here we have again the means of obtaining a conductor of accurately known length in an accurately known position relatively to the magnet, and the deflection of the magnet can be observed. The amount of deflection would be ascertained in this case, not by a graduated circle, but by means of a mirror which is attached to the magnet and turns with it, and the deflection of the mirror is obtained by means of a telescope. There is another apparatus here on the same principle. If we place a divided scale opposite the mirror, the divisions of the scale are reflected from the mirror back into the telescope which is placed opposite. In this way an extremely small deflection of the mirror, and, therefore, of the magnet, can be detected. The method of reading these two instruments is the same, but the first is an absolute instrument and the second is merely a comparative apparatus. It is a very convenient form of its kind. There are three pairs of coils belonging to it, one a pair of long coils of thin wire, and the other pairs being shorter and of thicker wire. There is the same arrangement as in the first I showed you for varying the distance between the conductor and the magnet.

Here again are other forms of galvanometers, but still comparative instruments only. These are different forms of Sir William Thomson's Reflecting Galvanometer, perhaps one of the most delicate instruments of the kind ever constructed to indicate the passage of an exceedingly small current. It will give us the comparative measurement of very weak currents, but not an absolute measure.

Here, again, is an apparatus, constructed by Professor Guthrie, which will also give us comparative measurements. It depends upon the mutual attraction or repulsion of little bits of iron magnetized by the passage of a current.

So much for the various ways in which electro-magnetic action can be made to serve as a basis for a system of measurement. It is much the most frequently employed principle, but we can also employ electro-dynamic action—

the force exerted by one current on another, or by one portion of a current on another portion of the same current. There are on the table two instruments which act on this principle. Professor Guthrie's may be regarded as one of this class. The same current passes round two pairs of small pieces of iron, and it is the action of one part of the current on another which really gives us the indication. Here, again, is an apparatus where the electro-dynamic action of the current is applied more simply, and without the intervention of the bits of iron. We have a circular coil of wire, to begin with, through which the current passes; then instead of suspending a magnet at the centre of the coil, there is another coil suspended inside, and the two coils are so set that when no current is passing, the planes of the separate portions of the wire are at right angles to each other. The wire in the fixed coil is in a plane parallel to the length of the table, but in the suspended coil the wire is wound almost at right angles to that direction. When it is properly adjusted, the directions of winding the two coils are at right angles to each other, but when a current passes through both of them there is a tendency in the two coils to set parallel to each other. If they are at right angles to begin with, a force which would tend to set them parallel, will cause a deflection, and the amount of this deflection, combined with a knowledge of the force resisting the displacement, and of the relative lengths and positions of the two parts of the conductors which act upon each other, gives us again the means of ascertaining the strength of the current. When we employ this principle the statement which involves the definition of the unit current may be put in this way:—Unit current placed around the circumference of a circle of unit area exerts upon an equal current, enclosing an equal area in a plane at right angles to the first, a couple whose moment multiplied by the cube of the distance between the areas surrounded by the currents equals unity when the distance is very great. In order to make the statement of the mechanical force simple, you must imagine that the two currents are at a very great distance from each other, as compared to the size of the area surrounded by either of them; the force exerted by one upon the other diminishes as the distance

increases, but, if you multiply the force by the cube of the distance, then the product approaches more and more nearly to a constant value as the distance becomes greater, and if the current is one of unit strength, and the two areas are equal to unity, the constant value to which this product approaches as the distance increases is unity. But, as you will see, the considerations involved are rather complex, and the further discussion of this subject would take much more time than we can spare.

Then another action produced by currents which might be adopted, and often is adopted, for measuring their strength, is the amount of chemical change which they can *produce in a given time; or, to express it more concisely*, the strength of a current can be measured by the rate at which it can produce chemical change in a body through which it is passed—by the rate, for instance, at which it can decompose water or any other chemical compound. Adopting this system we may take as the unit current one of such a strength that it will decompose unit mass of a standard substance—say water—in unit of time—say a second. We may define unit current as a current which will decompose a grain of water in a second, taking a grain as the unit of mass, water as the standard substance, and a second as the unit of time. But if we are to adopt this definition, the unit current obtained in this way would not agree with the unit current derived from the electro-magnetic action. An electro-magnetic unit current (referred to centimetres, grammes, and seconds), if used to decompose water, will liberate ·0001052 gramme of hydrogen per second, or it would decompose nine times that quantity of water. This gives us the ratio between the *electro-magnetic unit current* and this *electrolytic* unit. The apparatus employed for carrying out this kind of measurement is of various kinds. They are called Voltameters; many forms are familiar to all who have dealt at all with electrical processes, and I will, therefore, merely speak of one form which may be new to most if not all of you—a very beautiful instrument, constructed by Professor Lenz, of St. Petersburg, which Baron von Wrangel has kindly explained to me. It is an apparatus in which a basic mercurous nitrate is decomposed. The current is passed through two little glass vessels containing mercury,

communicating with each other by a narrow tube, the mercury in both vessels being covered with a solution of nitrate. The current passing causes more mercury to be dissolved in one vessel, and more mercury to be separated from the solution in the other, so that there is a continual carrying of mercury from one vessel to the other, by the action of the current. The rate of increase in the lower vessel, or, in other words, the quantity carried over in a given time measures the strength of the current. The apparatus is so arranged as to give the means of observing with very great accuracy the rate of increase in the quantity of mercury. There is a micrometer screw, by means of which the surface of the mercury can be lowered, and read off exactly against a mark, and then, after the experiment, the same measurement is made again, and the difference between the two readings of the screw indicates the increase in the volume of mercury. It is said that in the course of a minute or two a sufficient quantity of mercury is carried over to give a measurable increase, and that the measurement can be made with very great accuracy indeed. The essential electro-chemical principles involved are the same as in the ordinary voltameter, but there is a different mechanical arrangement for observing the amount of effect produced.

I have only time to state very briefly the general principles of the methods available for measuring Electromotive Force. In the most usual cases electric currents are produced either by chemical action or by the relative motion of magnets and conductors. In an ordinary galvanic battery we have chemical action taking place as long as the current passes, the quantity of the current which traverses the circuit being proportional to the quantity of chemical action which takes place. In apparatus, such as this on the table, we have currents produced by the motion of magnets and conducting wires relatively to each other. An absolute measure of electro-motive force may be derived from a study of the conditions under which currents are produced in either of these cases.

Thus, first, if we can determine the amount of energy expended in a galvanic battery when a unit of electricity traverses the circuit, we have really calculated the electromotive force. Now the quantity of heat corresponding to

the chemical action has in very many cases been determined, and when we know the quantity of heat, we have only to multiply this by the mechanical equivalent of heat, and we get the electro motive force; for the electro-motive force is the work done, or the amount of energy expended in the circuit when unit of electricity traverses the conductor. From the determination of the heat of chemical action, and the mechanical equivalent of heat we have the data for calculating the electro-motive force due to any given chemical process, such as takes place in a galvanic battery.

Again, if the magnetic forces acting upon a conductor which we move in a field of magnetic force are definitely known, and the motion of the conductor relatively to the magnetic field is definitively known, we can again calculate the quantity of work done in maintaining the motion, and hence also the electro-motive force produced. Thus the electro-motive force may be theoretically ascertained either from the energy of the chemical action required to produce the current, or from the work done in maintaining the motion of conductors in the neighbourhood of magnets; though I cannot enter into the details in either case.

When we have got a measurement of the current and of the electro-motive force, the measurement of Resistance follows from a comparison of the two. The electro-motive force acting in any circuit divided by the strength of the current which that electro-motive force produces, measures the resistance of the circuit. That is the general principle of all absolute methods of measuring the resistance of circuits. There is here an apparatus by which such a comparison can be made. This is a circular conductor which can rotate about a vertical axis, and as it rotates a current is produced in it by the earth's magnetic force. By suspending a little magnet inside this little box at the centre of the circle the strength of the current produced in the wire can be ascertained by the deflection of the magnet. The deflection of the magnet combined with the speed of the rotation and the dimensions of the coil gives us the absolute resistance of the conductor. This is the actual apparatus employed in a very elaborate series of measurements by a Committee of the British Association, of which Professor Clerk Maxwell was the most active member. It was employed

in determining the so-called British Association Unit of Resistance. Here is the coil with which the resistance of the revolving apparatus was compared periodically during the course of the experiments; and here are multiples of the standard so established, by ten, one hundred, and a thousand.

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